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# Essays on the Chinese and the U.S. Housing Markets

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# Essays on the Chinese and the U.S. Housing Markets

## **Abstract**

Housing is one of the most important assets for households and has profound implications for the economy. Housing markets in different nations may differ in institutional backgrounds and phases of housing cycles, but people across the world are faced with some similar challenges in understanding housing markets. This dissertation is international in scope and focuses on certain aspects of the housing cycles. It consists of two chapters -- the first looks at boom-taming housing policies in China and the second investigates the role of contagion in the previous U.S. housing cycle.

Chapter 1 examines the impact of major housing policy interventions in China. While research in the Chinese housing market has been hampered by severe data limitations, I propose turning to the stock market, where high quality and high frequency data on real estate firms are available. An event study analysis is conducted on the April 2010 central government announcement, which suddenly and sharply reversed prior policies and initiated efforts to cool the housing market by tightening mortgage credit supply. I find that publicly traded housing developers listed on the Shanghai, Shenzhen or Hong Kong stock exchanges suffered an average of -15% cumulative abnormal return (CAR) in a short event window around the policy announcement. This loss in firm value indicates that the policy intervention is well-received by the market. Transaction volumes are likely to decline in the short run and the steady-state house price appreciation rate is expected to drop as well. There also is noteworthy heterogeneity in the CAR, with firms that engage in some non-residential development performing about three percentage points better. Firms whose largest shareholder is a state-owned enterprise affiliated with the central government perform about five percentage points worse. This latter result provides useful insights into the relative magnitudes of the costs and benefits of having special connections to the central government.

In Chapter 2, which is written jointly with Anthony DeFusco, Fernando Ferreira and Joe Gyourko, we investigate whether contagion in the housing market, which is defined as the price correlation across space between two different metropolitan areas above and beyond that justified by common local shocks, was an important factor in the last American housing cycle. We implement empirical strategies that help address concerns that plague prior contagion-related research. Besides that, the richness of our proprietary housing transaction data allows us to directly estimate the importance of contagion mechanisms. We find that contagion effects arise during the housing boom, and only from the very closest neighbor -- the elasticity of focal market prices with respect to changes in its nearest neighbor's prices is in the range of 0.10-0.27. This is large enough to account for up to 30% of the jump in home prices at the beginning of local booms, on average. There is noteworthy heterogeneity in this result, with contagion impacts being much greater when transmitted from a larger to a smaller market, and also more important for the most elastically-supplied markets. Finally, local fundamentals and expectations of future fundamentals have very limited ability to account for our estimated effect. This suggests a potential role for non-rational forces in generating house price expectations.

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Joseph Gyourko

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ESSAYS ON THE CHINESE AND THE U.S. HOUSING MARKETS

Wenjie Ding

A DISSERTATION

in

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Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2013

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ESSAYS ON THE CHINESE AND THE U.S. HOUSING MARKETS

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Wenjie Ding

*To my family*

*Without whom this would not have been possible.*

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# Abstract

ESSAYS ON THE CHINESE AND THE U.S. HOUSING MARKET

Wenjie Ding

Joseph Gyourko

Housing is one of the most important assets for households and has profound implications for the economy. Housing markets in different nations may differ in institutional backgrounds and phases of housing cycles, but people across the world are faced with some similar challenges in understanding housing markets. This dissertation is international in scope and focuses on certain aspects of the housing cycles. It consists of two chapters – the first looks at boom-taming housing policies in China and the second investigates the role of contagion in the previous U.S. housing cycle.

Chapter 1 examines the impact of major housing policy interventions in China. While research in the Chinese housing market has been hampered by severe data limitations, I propose turning to the stock market, where high quality and high frequency data on real estate firms are available. An event study analysis is conducted on the April 2010 central government announcement, which suddenly and sharply reversed prior policies and initiated efforts to cool the housing market by tightening mortgage credit supply. I find that publicly traded housing developers listed on the Shanghai, Shenzhen or Hong Kong stock exchanges suffered an average of -15% cumulative abnormal return (CAR) in a short event window around the policy announcement. This loss in firm value indicates that the policy intervention is well-received by the market.

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# Chapter 1

## Evaluating Housing Policy

### Interventions in China Using Stock Market Data



## 1.1 Introduction

Understanding China's housing market and the impact of housing policy interventions has been an intriguing topic for scholars, policy makers, and investors. This is naturally explained by the country's importance in global economic growth and its recent extraordinary housing boom.<sup>1</sup> The Chinese government also intervenes in the housing market on a regular basis. Following a period of encouraging home purchase and real estate investment, the central government suddenly changed direction beginning in April 2010 to try to cool the housing market. New regulations were issued, which tightened credit supply for housing speculators via increases in minimum down payments and mortgage interest rates.<sup>2</sup>

Whether these regulations are effective in controlling housing demand and taming house price growth has profound implications for China. Housing is a large sector in the Chinese economy, with real estate investment accounting for 13% of China's GDP in 2011<sup>3</sup> and land sales being a crucial component of local public finance.<sup>4</sup> This question also is relevant for the well-being of Chinese households, about 90% of which own their homes.<sup>5</sup> Moreover, a potential slowdown in the housing market, if caused by these stringent regulations, is likely to cause global spillovers.<sup>6</sup> Despite all the above importance, economists know very little about the impact of the housing

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<sup>1</sup>For example, Wu, Gyourko and Deng (2012) document run-ups in land prices, house prices and price-to-rent ratios during 2008 and 2009, which can hardly be justified by market fundamentals.

<sup>2</sup>According to definitions by the Chinese government, housing speculators refer to: (1) buyers of multiple homes; or (2) buyers who do not live or work (pay taxes) in the cities in which they intend to buy homes.

<sup>3</sup>Source: the National Bureau of Statistics (NBS) of China.

<sup>4</sup>The state is the ultimate owner of urban land in China. Local governments sell land use rights via public auctions officially since 2002 (Cai, Henderson and Zhang (2009), Gordon and Li (2012), Deng, Gyourko and Wu (2012)). According to the NBS, 30%-50% of local government revenues come from land auction proceeds.

<sup>5</sup>According to China Household Finance Survey (2012), the home ownership rate in China reached 89.7%. This high home ownership rate, widely documented in other studies, is partly a result of the housing privatization reform in the 1990s which allowed employees to purchase homes that they had been renting from their state employers at lower-than-market prices (Wang (2011)).

<sup>6</sup>For example, Ahuja and Myrvoda (2012) suggest that a hypothesized decline in real estate investment would disrupt the production chain throughout the Chinese economy and cause negative spillovers to other G20 economies.

policy changes or their overall effectiveness.

A key reason is that empirical research about the Chinese housing market has been impeded by a lack of appropriate data. Publicly available house price series are not considered reliable.<sup>7</sup> The quality of micro-level transaction data also tends to be flawed because of the opaque nature of the resale and rental markets in China. In many cases, actual values of home transactions are not accurately observed. In addition, most cities do not have consistent housing data until 2006, leaving researchers a fairly short time-series with which to study market dynamics. Beyond data limitations, the way in which the Chinese government intervenes in the housing market also poses challenges in terms of research design. Major housing acts are decided by the central government and enforced nationally at the same time, which makes it difficult to find any exogenous variation across markets to help identify housing policy impacts. Another complexity arises from the fact that housing policies change frequently and sometimes switch directions. Therefore, if one must rely on data that takes a considerable time to collect or is released with a long lag, it can be difficult to tease out the effect of a single policy.<sup>8</sup>

In this paper, I propose a different strategy to evaluate recent changes in the Chinese housing policy that helps get around these limitations. Specifically, I turn to the equity markets where good quality and high frequency data is available on publicly traded real estate firms. A standard event study is conducted to determine whether and how policy interventions are capitalized into the market value of real

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<sup>7</sup>For example, the so-called 70-Cities Index, which is the most influential of the official house price series provided by the Chinese government, shows little volatility over time and little or no evidence of a boom in some big coastal markets widely believed to have boomed. Wu, Deng and Liu (2011) and Deng, Gyourko and Wu (2012) provide critiques of this series, along with others publicly available in China.

<sup>8</sup>These empirical challenges do not equally affect all possible policy analysis, of course. There are a few exceptions with appropriate data available and strong research designs feasible. For example, Wang (2011, 2012) provide quantitative estimates of the impact of housing privatization reform in the 1990s on both housing markets and labor markets using a panel dataset collected from household surveys. Cai, Henderson and Zhang (2009) collect urban land auction data in Beijing from 2003 to 2007 and compare the outcomes of different types of auctions for land-use rights.

estate firms. Because stock returns reflect expected future changes in firm cash flows, the extent of capitalization of the policy change into real estate firm value also can inform us about expected changes in housing market fundamentals.

The focus of my empirical work is the housing policy officially released on April 17, 2010 (commonly known as the State Council “Document Number 10”), which tightened mortgage lending criteria for housing speculators and marked the first surprising reversal from previously expansionary housing policies that had been in place since 2008.<sup>9</sup> Because this policy intervention was exogenous to firm value and was released without prior public anticipation, its announcement effect can be captured within a properly-defined short event window. Empirically, I find that real estate stocks responded quickly to the housing policy shift and in a manner that was consistent with the time line of the policy release. There was an average cumulative abnormal return (CAR) for real estate firms of about -15% until day 5 (relative to the date of policy announcement), which is an economically meaningful effect and stands up under various robustness tests.

If one is willing to make simplifying assumptions about the real estate firms and the underlying property markets in which these firms operate, this negative impact on firm value can help us anticipate how the policy will affect the housing market itself. I do so in an admittedly stylized way by considering the empirical implication from a dynamic housing model (Glaeser, et al. (2012)), which posits that house price in an area will grow at constant rate in steady state unless there is a shock to local fundamentals. Translated into this scenario, the well-received Chinese policy, which represents a shock to credit market conditions, would imply a decline in house price appreciation rate. While this exercise is not meant to be taken literally, it still is useful in helping us understand the range of housing price changes that are

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<sup>9</sup>Refer to the State Council “*Guowuyuan guanyu jianjue ezhi bufen chengshi fangjia guokuai shangzhang de tongzhi*” [Notice of the State Council to Resolutely Curb the Rapid Rise in Housing Prices in Some Cities], Guofa[2010], No.10. See Section 1.2.1 for more details on this policy.

consistent with a -15% CAR for housing developers. The calculations reported below indicate that the -15% housing-policy-induced abnormal return is consistent with an up to 10 percentage point drop in house price appreciation rate. Therefore, this April policy change well could prove effective in controlling housing demand and stabilizing housing prices, both of which seem to be key goals of the central government.

Within the housing literature, this paper also contributes to our understanding of how tightening down payment requirements affects housing demand and housing prices.<sup>10</sup> Credible estimates of this effect typically are difficult to produce because of the inherent endogeneity of leverage decisions in most contexts. In this case, I am able to examine an exogenous change in leverage requirements in China and provide reduced-form estimates of their impact. That said, one should be careful about generalizing these predictions to other countries because the mortgage market and credit policy in China have some distinctive institutional features.

Beyond documenting the average policy impact, I explore the heterogeneity in policy impacts across firms by whether they also engage in development of non-residential properties, by geographic exposure of their residential properties and by how politically connected the firms are. While these characteristics are presumed to affect intensity of the policy impact, estimates of their roles may be plagued by the endogeneity concern. To deal with this concern, I argue that identification is rested on the following facts: (1) the announcement of the policy is exogenous to firm value; (2) the policy intervention does not stipulate *ex-ante* rules that would favor some firms over others; and (3) there is no evidence of more speculative buyers sorting into a particular type of firms through the mechanisms being considered in this paper. When controlling for other firm characteristics and potential omitted variables, I

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<sup>10</sup>This topic has an international scope. Much academic work has focused on the role of interest rate and other credit market conditions in the U.S. housing cycle (for example, Himmelberg, Mayer and Sinai (2005), Glaeser, Gottlieb and Gyourko (2010), Mian and Sufi (2009, 2011), Favilukis, Ludvigson and Van Nieuwerburg (2010)). Ortalo-Magné and Rady (2006) show that both housing demand and housing price respond to changes in credit constraints. Geanakoplos (2010) argues the importance of managing leverage cycles in the housing market.

find that firms with some concentration in non-residential real estate development experienced an additional cumulative abnormal return of about 3%, which partially counterbalances the negative policy shock. Firms developing residential properties in speculative markets suffer an additional -4% abnormal return, but this effect is not statistically significant because most of the firms have geographically diversified portfolios. Finally, I detect an additional cumulative abnormal return of about -5% for firms whose largest shareholder is a state-owned enterprise affiliated with the central government. In this context, the expectation that those firms would be more vulnerable to a cutback in government support than private firms outweighs the usual benefits of political connections in buffering negative market shocks.

In sum, this paper is the first to show that equity market data can be used successfully to evaluate the impact of housing policy changes by the Chinese government, especially when appropriate data on the underlying market are not available or would be very costly to collect. In addition, the event study approach employed is shown to be valid in the Chinese policy context investigated. It also is insightful in that abnormal stock returns provide meaningful implications for future outcomes in the housing market, as well as the values of specific firm attributes that are important to real estate firms. Beyond the April housing policy intervention, the approach proposed in this paper can further be applied to a myriad of questions related to China's housing market and to examining the consequences and effectiveness of other policy interventions in sectors where primary data quality is suspect.

The rest of the paper is structured as follows. Section 1.2 introduces background. A description of data can be found in Section 1.3. Section 1.4 documents the abnormal announcement return of real estate firms in an event study and Section 1.5 talks about its implications for the underlying housing market. I then investigate the relationship between abnormal returns and firm attributes in Section 1.6. There is a brief conclusion.

## 1.2 Background

In this section, I introduce some basic facts about China’s housing market and recent housing policies, and then provide some background on publicly traded real estate firms.

### 1.2.1 The Housing Market and Recent Housing Policies in China

China has been in a housing boom for more than a decade. It is widely documented that the rising housing demand driven by urbanization and the tradition of favoring home ownership contribute to the boom.<sup>11</sup> The most dramatic acceleration in house price, however, occurred at the end of 2008. Both price-to-rent and price-to-income ratios took off at that time. While there could be multiple causes of this recent housing boom, one consensus is that the government-sponsored stimulus package issued in November 2008, which provided cheap loans to both real estate developers and home buyers, contributed to rising house prices thereafter (Wu, Gyourko and Deng (2012), Deng, Morck, Wu, Yeung (2011)). Such a credit expansion encouraged not only home purchases for consumption use, but also home investment by speculators.<sup>12</sup>

While no official measure exists in tracking housing speculation over time or across markets, several home vacancy studies help us gauge how speculative the housing markets are. In an electronic mapping project, the Beijing Police estimated that 29% of the homes in Beijing were unoccupied in June 2012.<sup>13</sup> While this number was

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<sup>11</sup>Urbanization is driving housing demand in China. A 1% increase in urban population growth among local *hukou* holders (i.e., residents) would lead to at least 0.54 percentage point house price appreciation between 2002 and 2008 (Wang and Zhang (2012)). In addition, the social norm in China favors home ownership and encourages households with a son to buy homes to improve their relative competitiveness in the marriage market (Wei and Zhang (2011)).

<sup>12</sup>Extensive research in the U.S. has suggested the important role of speculators in blowing up the prices during the boom and worsening the subsequent downturn by defaulting quickly and in large numbers (See Bayer et al. (2011), Campbell et al.(2011), Haughwout et al.(2012), Chinco and Mayer (2012)).

<sup>13</sup>See the news articles in *Global Times* and *Peoples Daily* (“Policy Gap Allows Hoarders to

disputed for including new units waiting to be sold, other onsite surveys also confirm that at least 20% of the already-sold units were apparently left unoccupied in selected residential complexes in Beijing and Shenzhen during the boom.<sup>14</sup> Speculators were active not only in superstar cities, but in second-tier and third-tier cities as well.<sup>15</sup> According to the 2011 China Household Finance Survey which covers twenty-five provinces in China, about 20% of the urban households own at least two homes.

In order to curb excessive growth in housing price and to tame housing speculation, the State Council of China unveiled three major waves of housing policies in 2010 and 2011. The leading policy, known as the State Council “Document Number 10”, was released on April 17, 2010.<sup>16</sup> This act is widely interpreted as the first reversal from the expansionary policies enacted during the 2008-2009 financial crisis.<sup>17</sup> The most substantial change in this policy involved tightening mortgage supply for housing speculators (Article #3 of “Document Number 10”). For example, the minimum down payment ratio for second-home purchases, which we consider as speculative purchases, was raised to 50% from 40%. It also called for mortgage rates for speculative purchases to be no less than 110% of the base rate. Non-residents can no longer obtain mortgages to buy homes in a city unless they have paid taxes in that city for at least one year. Figure 1.1 plots the minimum down payment rules for different types of buyers over time. First-home buyers are those who purchase homes for their own occupancy.<sup>18</sup>

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Undermine Property Curbs”, June 12, 2012).

<sup>14</sup>For example, the China Central Television aired two documentaries of on-site home vacancy surveys in Beijing (April 2010) and Shenzhen (April 2012). Their main approach was to count the number of lights at night and the number of people entering/exiting the neighborhoods during week days.

<sup>15</sup>One extreme example would be Sanya, a Miami-like tourist destination in Hainan Province. Nearly 85% of the residential properties were unoccupied according to the official data revealed by the local housing bureau in early 2012.

<sup>16</sup>The official document was disclosed on April 17, 2010, but its major contents were revealed to the public on April 15, 2010, by the Premier of China. Per below, I take April 15 as the announcement date of the housing policy.

<sup>17</sup>“The Turning Point in Housing” in *China Leadership Monitor* (vol.33, 2010) provides a good interpretation of the State Council “Document Number 10”.

<sup>18</sup>Both households buying their first homes and those buying new homes to upgrade their existing homes belong to the category of “first-home buyers”.

According to Figure 1.1, the April policy further distinguishes speculators from first-home buyers, and represents the first shift from previous regulations.

Besides the central focus of regulating mortgage supply to speculators, “Document Number 10” specified the responsibilities of local governments to curb house price growth, to guarantee effective supply of housing (Article #1, 2, 5, 6) and to construct government-sponsored housing units (Article #7). It also strengthened supervision over housing developers and punishes violators more severely (Article #8, 9). Appendix Table 1.A has a detailed description of this policy.

The State Council made two subsequent policy announcements on September 29, 2010, and January 26, 2011, both of which stepped up their initial efforts to tighten mortgage lending policies.<sup>19</sup> The September policy required all first-home buyers to pay 30% of their transaction values upfront, and it stopped providing mortgages to third-home buyers. The January policy further raised the minimum down payment to 60% for second-home buyers and imposed home purchasing limits on urban households.<sup>20</sup>

In this paper, I focus on the April housing policy because it was the first policy reversal from expansionary policies during the financial crisis and was a real surprise. The two follow-up policies added only marginal changes to the April policy and were likely to have been anticipated to some extent. Section 1.4.4.4 has more discussions on this. Before moving on to the data, the following subsection provides some background on publicly traded real estate firms in China.

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<sup>19</sup>Refer to the State Council “*Guojia youguan buwei chutai cuoshi ezhi bufen chengshi fangjia guokuai shangzhang*” [Measures on Curbing the Rapid Rise in Housing Prices in Some Chinese Cities] (September 29, 2010) and “*Guowuyuan bangongting guanyu jinyibu zuohao fangdichan shichang tiaokong gongzuo youguan wenti de tongzhi*” [Notice of the State Council to Further Tighten the Regulation of China’s Real Estate Market] (January 26, 2011).

<sup>20</sup>In general, households satisfying the following criteria are not allowed to buy any more homes in a city: (1) city residents who already own two homes; (2) non-residents who already own one home; and (3) non-residents who have not paid taxes in that city for a certain number of years.



### 1.2.2 Publicly-Traded Real Estate Firms in China

The largest real estate firms in China are publicly traded. They are largely listed on the Hong Kong Stock Exchange (HKEx) as well as two mainland stock exchanges – the Shanghai Stock Exchange (SSE) and the Shenzhen Stock Exchange (SZSE). Table 1.1 shows how representative publicly traded real estate firms are along various dimensions. Information about publicly-traded real estate firms is calculated based on the sample adopted in this paper (see Section 1.3 for details) and data on all real estate firms are available on an annual basis from the National Bureau of Statistics (NBS). According to Table 1.1, publicly traded real estate firms constitute no more than 1% of all firms in the industry, but they represent 7%-8% of the total assets and approximately 14% of the market share nationwide in terms of sales revenue. Beyond those numbers, publicly-traded real estate firms also remain the most active and reputable firms in the real estate industry.<sup>21</sup>

It is worth noting that Table 1.1 is likely to under-estimate the importance of publicly traded real estate firms because the NBS adopts a broader definition of real estate firms and may include project-based firms, which are temporarily set up for some housing projects and become inactive after the projects are finished. In addition, figures in Table 1.1 represent national averages and may undervalue the market share of publicly traded real estate firms in more established cities where those firms operate.

Overall, publicly traded real estate firms represent major and active players in the housing market. Their behavior and performance give us a good picture of the real estate industry and the housing market in China.<sup>22</sup>

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<sup>21</sup>For example, 9 of the top 10 real estate firms in China are publicly traded according to SouFun Real Estate Enterprise Research in 2011.

<sup>22</sup>Unless otherwise mentioned, “housing market” in this paper exclusively refers to the private housing market, in which more than 60% of the homes transacted involve newly-constructed units sold by real estate firms. The performance of public (i.e., government-sponsored) housing market is beyond the scope of this paper.

## 1.3 Data

I start with all publicly traded firms on the Hong Kong Stock Exchange (HKEx), the Shanghai Stock Exchange (SSE), and the Shenzhen Stock Exchange (SZSE) because the affected real estate firms (i.e., the treatment sample) are typically listed on one of the three stock exchanges. The Shanghai and Shenzhen Stock Exchanges are two mainland stock exchanges supervised by the China Securities Regulatory Commission (CSRC) and are available mostly to domestic investors. The Hong Kong Stock Exchange attracts both local and foreign investors and is effectively an overseas stock exchange subject to independent regulation.<sup>23</sup>

The treatment sample of interest is all publicly traded real estate firms that develop residential properties in mainland China, which are directly affected by the housing policy interventions.<sup>24</sup> I develop the treatment sample from the official list of real estate firms provided by each stock exchange. I pick the category “J01” (Real Estate Development and Operation) firms listed on the SSE and SZSE according to the CSRC industry classification system. For firms listed on the HKEx, I choose the “Real Estate” category according to the Hang Seng Industry Classification.<sup>25</sup>

Going through company profiles and financial reports over time, I collect information about each firm’s business lines, IPO date and any business switch since its IPO. I then apply the following restrictions to address various concerns in the event study.

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<sup>23</sup>In this paper, I only consider the A-share equity markets of the SSE and SZSE. “A” shares are priced and traded in Renminbi and are largely available to mainland investors in China. Foreign investors are approved to trade “A” shares under some quotas. The B-share markets of the SSE and SZSE are open to foreign investors trading in U.S. dollars and Hong Kong dollars, respectively, and they are much smaller than the A-share markets in terms of total market value and turnover value. There are six real estate firms in the sample cross-listed on both A-share and B-share markets, but less than 10% of their total shares are issued on the B-share market. Throughout this paper, the results for the B-share stocks are not reported along. However, the performance of B-share real estate stocks is very similar to that of their A-share counterparts.

<sup>24</sup>In this paper, I also examined a placebo sample of firms outside the real estate industry. See Section 1.4.4.3 for details on sample construction.

<sup>25</sup>The industry classification code is updated and overwritten frequently. I collected the code in April 2012, and double checked with firm information further back in time to make sure that the 2012 industry code reflects the firm status in 2009 and 2010.

First, I require that firms maintain their principal business in mainland China, so that they are directly subject to the housing policy interventions and barely subject to other region-specific shocks. Thus, real estate firms operating locally in Hong Kong and publicly traded on the Hong Kong Stock Exchange are not included in the treatment sample. Second, I require that the real estate firms do not participate in other industries outside real estate. Otherwise, the stock response to the housing policy shift would be confounded by other contemporaneous industry-specific shocks. Third, each firm in the sample should have data since January 1, 2009, so that I have a long enough time-series to estimate individual stock returns. More specifically, firms that had an IPO or transformed themselves into real estate firms after January 1, 2009 are not included in this paper.

Table 1.2 has a brief summary of the sample. About 4%-6% of all publicly traded firms on the three stock exchanges are identified as real estate firms. They are above the median in terms of market value, which is consistent with the fact that publicly traded real estate firms in China are large. The final treatment sample consists of 129 firms: 46 publicly traded on the Hong Kong Stock Exchange, 45 on the Shanghai Stock Exchange and 38 on the Shenzhen Stock Exchange. Daily stock prices and returns (without dividends) for each firm are obtained from the China Securities Market and Accounting Research (CSMAR) database and Bloomberg.

## **1.4 The Housing Policy and Real Estate Stock Returns**

I begin the empirical analysis with examining whether the policy intervention to cool the housing market results in negative stock returns for real estate firms.

### 1.4.1 Methodology

In order to account for market-wide movements, I use the classical event study approach to analyze abnormal returns of real estate stocks in response to the housing policy. Several institutional features of the April policy make it well-suited to an event study analysis.

First, this policy is exogenous to real estate firms. It is a decision of the State Council Standing Committee, not triggered by any particular firm or industry. In many countries, one would naturally worry about lobbying, especially by firms with strong political connections. They might talk to policy makers and try to influence them to make decisions that favor those particular firms. Although there is no reason to rule out this possibility, it is less likely to happen in this context. First, the housing policy amounts to a negative shock and is a bad news to firms. Firms do not have strong incentives to persuade decision makers to carry out a policy that works against their own interest. In addition, the housing policy does not specify anything that would differentially “protect” some real estate firms than others. The only quantifiable article of this policy is to tame speculative demand. As such, there is no evidence of firms manipulating specific rules and cutoffs to avoid being punished.

Second, like other top-level policies in China, the housing policy intervention was not leaked to the public beforehand via channels unobservable to economists. Individuals would be jailed for leaking confidential policies or economic data ahead of their official release and enabling insiders to profit. Thus, the policy effect can be captured within a short window around the policy announcement. This differs greatly from many cases in the U.S. or in other democratic countries, where public debate may cause the stock market to respond long before any bills get approved.

Besides these institutional conventions, the Real Estate Climate Index of China offers another piece of evidence to bolster the argument that the public did not know the housing policy before its announcement. This monthly climate index reflects

overall conditions and prospects for housing sector.<sup>26</sup> According to Figure 1.3, the index had been trending up since early 2009 and did not show any signs of decline until April 2010. Therefore, the housing market was expected to be improving until the surprising onset of the April policy shift.

It is also worth noting that the “no prior information leakage” assumption does not exclude the possibility that there may be official notification shortly before the actual policy announcement. Fortunately, the exact dates of all previous announcements can be clearly identified in China. Thus, we can recover the full policy impacts accurately by flexibly defining event windows to embrace prior notices. In this particular case, the head of China’s Banking Regulatory Commission made a public speech at the Boao Asia Forum four calendar days before the official policy announcement and this event well could have caused stock market reaction in advance. Sections 1.4.3 and 1.4.4 below examine this directly. If anything happens even prior, my estimate would reflect a lower bound of the real impact of the April policy.

Third, the fact that the April policy represents a sudden policy reversal and that it was not correlated or coordinated with other macro events validates the interpretation of it being the source of any abnormal returns detected. This, however, may not be applied to some of the other Chinese housing policies, which were either riddled with ambiguity or were released together with other economy-wide policies. In those cases, it would be difficult to tease out the particular effects coming from housing policy interventions.<sup>27</sup>

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<sup>26</sup>The index is available from the National Bureau of Statistics. It is calculated at the national level by drawing information from monthly statistics of real estate investment, sales and development. A reading above 100 is considered to reflect good conditions.

<sup>27</sup>Section 1.4.4.3 addresses the concern of confounding events. This paper also evaluates other recent housing policy interventions during 2008 and 2011, in which the announcement effects are either not significant or small in magnitude. See Section 1.4.4.4 for details.

### 1.4.2 Empirical Framework

Following conventions in the literature, I use the multivariate regression framework in which abnormal return is parameterized in an individual stock return equation.<sup>28</sup>

To deal with the concern that the overall market may be affected by the April policy, I estimate risk-adjusted returns:

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \gamma_{i,k} D_{t,k} + \varepsilon_{i,t} \quad (1.1)$$

where  $t$  is the relative trading day to the announcement date of the housing policy;  $R_{i,t}$  is the daily stock return of firm  $i$  between closing prices at day  $t$  and day  $(t - 1)$ ; and  $R_{m,t}$  is the daily return of the market portfolio.<sup>29</sup> The dummy variable  $D_{t,k}$  takes on a value of 1 if day  $t$  falls in the event window starting since day -20 until day  $k$  ( $k \in [-20, 20], k \in Z$ ):

$$D_{t,k} = \begin{cases} 1 & \text{if } -20 \leq t \leq k \\ 0 & \text{if otherwise.} \end{cases}$$

Figure 1.2 illustrates the time line of the event study analysis. Day 0 is taken as April 15, 2010, which is the first time the policy was released to the public by the Premier of China (April 17 is the date when the official policy document was disclosed). The whole estimation period starts from January 1, 2009 and ends 20 days after the official announcement. Analogous to the traditional residual analysis approach (Campbell et al.(1997), MacKinlay (1997)),  $\gamma_{i,k}$  is interpreted as the average daily abnormal return over the  $[-20, k]$  event window for firm  $i$ . Accordingly, the firm-

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<sup>28</sup>See Binder (1985), Dube et al (2011), etc. I also follow the procedure outlined in Campbell, Lo, and MacKinlay (1997), which calculates abnormal return as the deviation of actual return from predicted return during the event window. This traditional residual analysis produces similar results to those from the regression method, but the former does not account for correlated errors in the cross section, which could be a serious concern in this setting because all firms are in the same industry and the event time completely overlaps.

<sup>29</sup>I use the composite index of the Shanghai and Shenzhen Stock Exchanges, and the Hang Seng Index of the Hong Kong Stock Exchange when calculating market returns of each stock exchange.

specific cumulative abnormal return is calculated as:  $CAR_i[-20, k] = (k + 21) \cdot \gamma_{i,k}$ .

There is no definitive rule regarding the choice of event window in the literature. In this paper, I start the event window 20 trading days before the policy announcement in order to detect any potential pre-trends caused by prior notifications. Ignoring pre-trends would bias the estimates of the housing policy impact toward zero. The same window length was used in other studies of corporate events such as earnings announcements (MacKinlay (1997)). To the extent that the results may be sensitive to the choice of window length, I experiment with alternative windows, such as  $[-10, k]$  and  $[-5, k]$ . See Section 1.4.4.1 for more details on this.

The return equations are jointly estimated for all firms using generalized least squares. This accounts for the fact that the abnormal returns are likely to be contemporaneously correlated when firms are in the same industry and experience the same event.

### 1.4.3 Baseline Estimates

To examine the overall stock response to the housing policy, I test the null hypothesis that the average abnormal return among real estate firms is equal to zero during each event window  $[-20, k]$ . I sequentially extend  $k$  from day -20 to day 20 to investigate the dynamics of abnormal returns during the entire course of the policy announcement.

$$H_0 : \quad \overline{CAR}[-20, k] = \frac{1}{N}(k + 21) \sum_{i=1}^N \gamma_{i,k} = 0, \quad (k \in [-20, 20], k \in Z)$$

#### 1.4.3.1 Full Sample Result

Table 1.3 Column (1) shows the baseline results for the full sample of 129 real estate stocks. Average cumulative abnormal return (CAR) and its asymptotic standard

errors are reported. The null hypothesis being rejected at conventional significance levels are indicated with stars. Results for only selected event windows are reported for space reasons. Correspondingly, the complete schedule of average CAR and its 95% confidence intervals are plotted in Figure 1.4.

Not surprisingly, real estate stock returns react negatively and timely to the April housing policy shift. From day -20 to day -5, average cumulative abnormal return is not distinguishable from zero, but it turns negative and significant starting at day -4. It then rapidly accumulates between day -3 and day 5 and reaches -15.5% over the [-20,5] event window. In the next 15 days following day 5, the negative response does not seem to go away quickly, with average CAR bouncing around -15%. However, this paper does not focus on this persisting trend. As is mentioned in the literature, the interpretation of abnormal stock returns over the longer run is more likely to be contaminated by noise (Campbell, Lo and MacKinlay (1997)). Throughout this paper, I interpret the -15% abnormal return (until day 5) as the response triggered by the April policy intervention.

Another important finding is that the negative stock response is detected approximately three days in advance, and this pre-trend can be justified by a particular event as briefly mentioned in Section 1.4.1. Figure 1.2 sketches the time line of all housing-related events around the announcement of “Document Number 10.”<sup>30</sup> On April 11 (the non-trading Saturday before day -3), the former chairman of China’s Banking Regulatory Commission, Liu Mingkang, made a speech at the Boao Forum for Asia (BFA).<sup>31</sup> He warned banks of mortgage lending risks and urged banks to tighten lending policies for housing speculators by raising down payment require-

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<sup>30</sup>I collect the dates on which those events were reported from the Xinhua News Agency, which is the largest official press bureau in China. Since the Xinhua Agency is also responsible for censoring important news to be released in China, no other media outlets are likely to take the lead. Thus, the dates I collect should accurately document when each of the events was announced to the public.

<sup>31</sup>This non-governmental Forum is one of the most influential forums for leaders in government, business and academia in Asia and other continents to share visions on the most pressing issues. The BFA is held annually in Boao, China. It is an important event for which major newspapers would reserve special columns.



ments and mortgage interest rates. Although the speech was non-governmental (i.e., unofficial) in nature, it was largely in line with the mortgage policies stipulated later in “Document Number 10,” and was made known to the public through major newspapers. Therefore, it can account for the pre-trend of a negative CAR, which becomes significant at the 1% level since day -3. In addition, the special sessions on China’s housing market since the start of the BFA on April 8 are likely to explain the modestly negative CAR around day -5.

It is also striking that the negative cumulative abnormal return does not accumulate evenly over time. The most dramatic plunges of the CAR happen exactly at the time when there are important information disclosures. To be specific, the majority of the -15% CAR over the  $[-20,5]$  window is realized immediately after the two critical events on April 11 and April 17, while the rest gets accumulated in between. For example, the next-day (day -3) abnormal return is about -3.5% in response to Chairman Liu’s speech on April 11. After the official document of the policy was released to the public on April 17, the next two trading days (day 2 and day 3) experience an abnormal return of -6.5%. Noticeably, real estate stocks respond to housing-policy-related events in a timely manner.

#### **1.4.3.2 Stock Responses across Stock Exchanges**

To the extent that the Hong Kong and the two mainland stock markets have different institutional features and attract different types of investors, the housing policy may have a differential impact on HK-listed and mainland-listed firms. Columns (2) and (3) of Table 1.3 report the average cumulative abnormal return for real estate firms publicly traded on the HKEx and the two mainland stock exchanges, respectively. Figure 1.5 provides graphical evidence.

While negative reactions are universally documented, mainland-listed firms tend to respond earlier than HK-listed firms. The negative response of mainland-listed

firms becomes significant on day -5 (April 8) possibly due to the housing-related discussions at the Boao Forum. HK-listed firms, however, react only modestly on day -3 to Mr. Liu’s public speech. The negative CAR does not become significant until around day 0.

The difference in the timing of stock responses across stock exchanges could be explained by how the policy information becomes known to investors. First of all, the policy news may be broadcasted more intensively in mainland, but may come with a lag or with some loss in translation to Hong Kong investors. Moreover, mainland investors, who invest mostly via the two mainland stock exchanges, may be more sensitive to mainland policy changes.

Similar to the full sample findings, two discrete plunges in the CAR are detected among both HK-listed and mainland-listed firms, which account for the majority of negative responses over the event window. The one-day abnormal return in response to Mr. Liu’s speech is -2.3% for HK-listed firms and -4.2% for mainland-listed firms. After the official release of the policy document on April 17, the immediate abnormal return over day 2 and day 3 is approximately -5% and -7% for HK-listed and mainland-listed real estate firms, respectively.

#### **1.4.4 Robustness Tests and Extensions**

So far, I have estimated an average cumulative abnormal return of around -15% in response to the announcement of “Document Number 10”. In this section, a few robustness tests are performed to lend support to the magnitude and interpretation of this estimated impact.

##### **1.4.4.1 Alternative Event Windows**

One of the concerns in any event study analysis is the choice of event window. Short windows around event dates are widely adopted in the literature, but there is no

definitive way of picking the “correct” length of window. Should the baseline findings be sensitive to this, I repeat the analysis with alternative event windows starting on day -10 and day -5. Table 1.4 compares  $\overline{CAR}[-10, k]$  and  $\overline{CAR}[-5, k]$  with the baseline results for the full sample of firms.

The average CAR exhibits similar patterns from columns (1) to (3). Negative and significant response is detected approximately 4 days before day 0 and accumulates after that. More encouragingly,  $\overline{CAR}[-10, k]$  stays pretty much the same as  $\overline{CAR}[-20, k]$ , which is consistent with the fact that there is basically no abnormal return before day -10. However, starting the event window from day -5 produces smaller estimates in magnitude, because the negative (though not significant) response around day -5 is not fully captured.

Thus, the baseline findings as discussed in Section 1.4.3 hold regardless of the specification of event windows. Extending the event window further back in time has the strength of detecting pre-trends and capturing stock responses to the housing policy more accurately. Per below, I always use the event window starting from day -20 in the baseline analysis and use other windows as robustness checks.

#### 1.4.4.2 Time-Shifted Placebo Events

To affirm that the policy effect is not a coincidence driven by local patterns in the data, I re-estimate Equation (1.1) on a set of placebo events. I construct the placebo events by randomly shifting the actual event date (April 15, 2010) backward as well as forward in time by 20, 30, 40, 50, 100 and 150 days. I then estimate the 5-day average cumulative abnormal return ( $\overline{CAR}[-2, 2]$ ) around each placebo event.

I report the results in Table 1.5. None of the placebo events shifted backward in time cause significant abnormal returns. Only the two placebo events shifted 40 days and 50 days after the real event have negative CARs, which are significant at the 5% level. But neither of them has a magnitude above 1.5%.

The placebo event estimates support the argument that the baseline policy effect is not obtained by chance. The pattern of no abnormal response before the policy and sizable abnormal return immediately after the policy is consistent with the hypothesis of the housing policy causing a decrease in the value of real estate firms. In addition, the mild and negative abnormal return in the longer run after the policy, though not definitively predicted by theory, is also reasonable and could be a result of a feedback effect.

#### 1.4.4.3 Non-Real-Estate Placebos

While the benchmark findings consider negative abnormal returns experienced by real estate firms as the impact of the housing policy, this interpretation may be confounded by other potential events that occurred around the same time. For example, there could be unusual upswings in other sectors around the housing policy announcement which left the real estate industry behind. It is also likely that some contemporaneous events (aside from housing policies) disproportionately depressed the real estate industry relative to others. If either is the case, the estimated abnormal returns would exaggerate the real housing policy impact.

To gauge the extent to which this is true, I run placebo tests on non-real-estate firms. Intentionally, the candidate placebos do not include sectors that are closely linked to or dependent on the real estate industry (for example, construction, banking, steel and lumber). In theory, they are likely to be direct victims of the housing policy shift.<sup>32</sup> With this filter applied, I randomly select eight industries from both the two mainland stock exchanges and the HKEx.<sup>33</sup> These industries include agriculture, manufacturing, mining, social services, etc.

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<sup>32</sup>Analysis on those industries finds that the construction and banking industries respond in similar manners as the real estate industry. However, abnormal responses are not detected in the steel and lumber industry immediately after the policy announcement.

<sup>33</sup>The placebo analysis is done separately for the two mainland stock exchanges and the Hong Kong stock exchange because they have different industry classification rules.

One might argue that the placebo industries would also be affected by the housing policy shift via common shocks to the economy. For example, since the housing policy forecasts a deceleration of house price growth and the housing sector is a key contributor to China’s GDP, investors may worry about the overall economic outlook of China and devalue Chinese firms in other industries as well. This scenario, however, should be picked up by the market beta of each firm. Above and beyond co-movements with the market, there is little evidence of any pair-wise dependence between a placebo industry and the real estate industry. Therefore, by default, we should not expect any housing-policy-induced abnormal reactions among placebo firms.

Following the same procedure, I estimate abnormal returns for the placebo groups. Table 1.6 and Table 1.7 report average cumulative abnormal returns ( $\overline{CAR}[-20, k]$ ) at a 5-day interval for each placebo industry publicly traded on the Hong Kong Stock Exchange and the two mainland stock exchanges, respectively. Figure 1.6 and Figure 1.7 provide graphical evidence paralleling the tables. Not surprisingly, hardly any significant abnormal return is detected among the placebo groups during the  $[-20, k]$  windows, in particular between day -5 and day 5 when the housing policy was released. The agriculture and mining industries of the two mainland stock exchanges may experience non-zero abnormal returns, but only randomly without noticeable trends.<sup>34</sup> And the magnitudes of their abnormal returns never exceed 5%.

These placebo estimates are consistent with the hypothesis that the stock returns of firms that do not have direct business connections with the real estate industry should not respond abnormally to the housing policy shift. This finding also provides support for the claim that abnormal returns of real estate firms reflect just the effect of the housing policy intervention, rather than that of other confounding events.

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<sup>34</sup>Abnormal returns among those industries may be caused by the release of CPI at the end of April 2010. For example, the rise in pork prices led to positive abnormal return of agriculture firms after day 15.

#### 1.4.4.4 Other Policy Events

In addition to “Document Number 10”, I evaluate three other recent housing policy interventions using the event study method. Average cumulative abnormal returns ( $\overline{CAR}[-10, k]$ ) are plotted in Appendix Figure 1.A.

The first is an expansionary policy, which was announced on December 20, 2008, to boost the housing market.<sup>35</sup> It aimed at encouraging home purchase and real estate investment. Not surprisingly, positive abnormal return is detected around this announcement. However, it is significantly distinguishable from zero during the entire window. As mentioned in Section 1.4.1, this policy was introduced along with many other monetary and fiscal policies during the same time. Thus, it is difficult to separate out the housing policy impact.

Appendix Figure 1.A(ii) represents the April policy, while (iii) and (iv) refer to the two policies announced on September 29, 2010, and January 26, 2011, as introduced in Section 1.2. Not surprisingly, the April policy reversal brought about the largest stock response among the three negative policy interventions after 2008. Investors barely responded to the September 2010 policy, probably because it was basically a supplement to the April policy and was not a big surprise. The January 2011 policy was associated with significant abnormal return of around -5%. Besides being another supplement to “Document Number 10”, it introduced for the first time purchasing limits on households as a measure to tame speculators.

## 1.5 Implications for the Housing Market

So far, I have documented a -15% abnormal change in the market value of real estate firms in response to the announcement of “Document Number 10.” The next question

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<sup>35</sup>Refer to the State Council “*Guowuyuan bangongting guanyu cujing fangdichan shichang jiankang fazhan de ruogan yijian*” [Several Comments on Promoting the Healthy Growth of the Real Estate Market], Document No.131, December 20, 2008.

to explore is what this -15% abnormal return tells us about expected future outcomes in the housing market where those firms operate.

### 1.5.1 Implied Policy Impact on the Housing Market

One noteworthy implication of the -15% abnormal return is that the policy intervention was well capitalized. To the extent that stock prices reflect rational expectations of future changes in fundamentals, this negative stock response well could imply gloomy prospects in the underlying housing market as perceived by investors. The negative sentiments circulated among the stock market would then affect housing investors' expectations about future house price growth and lead to further declines in housing price.<sup>36</sup>

Here, I start by discussing some potential outcomes in the housing market. First and foremost, the April policy intervention implies a shock to housing demand. As is discussed in Section 1.2, it tightened credit supply for speculative home buyers and would directly suppress the demand of speculators whose budget constraints become binding after the policy.<sup>37</sup> Affluent speculators and first-home consumption buyers may also adjust their views about the housing market and change their home purchasing decisions. All together, the April policy would imply a negative shock to housing demand.

In the short run, house price is widely documented to be sticky downward with sales falling first in response to a negative demand shock (Case and Shiller (1989), Case (2008)). No similar pattern has been documented in China in the literature

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<sup>36</sup>The recent literature has emphasized the importance of behavioral factors in the U.S. housing boom, for example, Case and Shiller (2003), Case, Shiller and Thompson (2012).

<sup>37</sup>In practice, marginal housing speculators did respond to the tightened mortgage policies by increasing their down payment. For example, the average down payment ratio of second-home buyers increased from 56% to 64% two months after the January 2011 housing policy, which raised minimum down payment to 60% from 50% for second-home purchases (According to the Shanghai Banking Regulatory Commission, that tracks the mortgage lending activities of four anonymous banks in Shanghai from January to March, 2011). Unfortunately, relevant statistics before and after the April 2010 policy shift are not reported.

because the time series is not long enough to observe a complete housing cycle. However, the downward stickiness of housing prices is equally likely to be true in China. In addition to commonly-cited explanations such as search cost and loss aversion (e.g., Genesove and Mayer, 2001), the fact that housing developers in China face a menu cost of lowering prices once the sales start also contributes to the inertia in housing price. Therefore, the demand shock implied by the April intervention would be likely to bring down transaction volumes in the short run, while home prices remain sticky but possibly grow at a lower rate. In the longer run, housing developers may lower their sales price to clear the inventory.

In order to interpret the policy shock to housing supply, it is important to notice that the aggregate supply of housing is determined by the Chinese local governments, with the latter deciding the amount of urban residential land supply and the floor area to be built on each land parcel under the guidelines of the central government and central land bureaus. Therefore, the local governments are the ultimate “monopoly” suppliers of the housing market. Since the April policy urged local governments to “guarantee effective supply” of urban land for residential use, it is most likely that the shock to supply would be neutral or positive.<sup>38</sup> However, a positive supply shock, i.e. an outward shift of the supply curve, would put further downward pressure on home prices.

To briefly sum up, the announcement of “Document Number 10” generated phenomenal market response. Transaction volumes are expected to decline in the short run. In the long run, the momentum of house price growth is likely to slow down.

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<sup>38</sup>See Article #5 and #6 of “Document Number 10” (Appendix Table 1.A). In fact, land supply is subject to local geographic, economic and regulatory concerns. It is also related to local political economy, i.e., local governments may have different incentives from the central government since 30%-50% of the local revenues come from land auction proceeds. In this paper, I abstract from all these concerns and assume that local governments would follow what was stipulated in the “Document Number 10”.



### 1.5.2 A Stylized Theoretical Framework

Beyond the potential consequences discussed above, one would still be interested in knowing the magnitude of housing price change that is consistent with the -15% abnormal return among real estate firms. The typical way to address this question is to write down a dynamic housing equilibrium model, simulate the time series of housing market outcomes with and without the policy intervention, and then assume the future path of market outcomes is capitalized into real estate stock returns. We could then back out the magnitude of expected housing market changes using the estimated abnormal return. In this case, however, a dearth of housing and urban data makes this approach impossible to carry out.

To shed some light on this question, I appeal to the dynamic spatial equilibrium model developed in Glaeser, et al. (2012). One of the intuitions from their model claims that house prices grow at a constant rate ( $\alpha$ ) in steady state. In this setting, a demand shock implied by the policy intervention would shift the equilibrium so that house prices grow at a lower rate ( $\alpha'$ ) in the new steady state.<sup>39</sup>

**A Simple Calculation** Now let's consider the market value of real estate firms. Suppose the housing market is populated by a continuum of identical real estate firms of measure one. The Chinese government is the “monopoly” supplier in the market, and firms are participants who split the monopoly profit by producing and selling similar housing units in equilibrium. If each real estate firm operates over  $T$  years, its market value can be written as the discounted value of all its future profits from housing development:<sup>40</sup>

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<sup>39</sup>Here, we discuss a rational expectation model where the existence of housing bubble is not incorporated.

<sup>40</sup>This approach is adapted from the finance literature on future commodity prices and corporate value, which presents discounted-cash-flow-based or option-based models to value mining and oil companies (Tufano (1998), Blose and Shieh (1995), Jin and Jorion (2006), etc.). The underlying idea is that changes in commodity price levels lead investors to change their perceptions of future cash flows and adjust the perceived value of firms.

$$V = \sum_{t=1}^T \frac{\pi P_t Q_t}{(1+r)^t} \quad (1.2)$$

where  $P_t$  = equilibrium housing price in year  $t$ ;  $Q_t$  = equilibrium amount of housing supply in year  $t$ ;  $\pi$  = profit margin per unit;<sup>41</sup> and  $r$  = discount rate.

Suppose in the original steady state,  $P_t = (1 + \alpha)P_{t-1}$ . After the policy intervention, house price follows  $P_t = (1 + \alpha')P_{t-1}$ . Based on discussions in Section 1.5.1, I assume that the steady state aggregate housing supply does not change for simplicity.<sup>42</sup> If we write down an expression for the market value change of real estate firms ( $\Delta V/V$ ), we can then back out numerically the change in expected house price growth ( $\Delta = \alpha' - \alpha$ ) by imposing  $\Delta V/V = -15\%$ . Under various assumptions of underlying parameters, this -15% abnormal return is consistent with a 3-9 percentage point drop in expected house price appreciation. Figure 1.8 has more details.

### 1.5.3 Discussions

Next, I provide some evidence of changes in transaction volumes and house prices after the April policy intervention. It helps us gauge effectiveness of the policy intervention, but this exercise should not be interpreted as matching moments in data with the stylized model predictions presented above.

Table 1.8 lists the summary statistics of floor area of housing sold in 35 major Chinese cities from 2000 to 2011.<sup>43</sup> As we can see, the housing policy shift was followed immediately by a drop in housing transaction volumes in 2010 and 2011 across

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<sup>41</sup>Here, I assume that the variable cost of home production, such as land and construction cost, can be expressed as a proportion of house price. This expression does not include firm fixed cost, such as the overhead expenses to obtain loans, licenses, permits, and etc. Technically, the April policy intervention tightened regulations on developers and may raise the fixed cost of firm operation (Article #8 and #9 of “Document Number 10”, see Appendix Table 1.A). In the calculation below, I abstract away from this story and assume that the value decline of real estate firms only comes from changes in house prices and quantities.

<sup>42</sup>An increase in supply, which is likely to be the case after “Document Number 10”, would amplify the slowdown in house price growth.

<sup>43</sup>This information is available from the National Bureau of Statistics.

the nation. It brought about the second decline in average housing transactions in a decade (the first was triggered by the 2008 financial crisis). Average floor area sold declined by 6% in 2010 compared to a year earlier and it continued to decline, though less abruptly in 2011. Sixteen markets experienced sales declines in 2010, and thirteen of them had double-digit decline.<sup>44</sup> Figure 1.9 plots the floor area sold for five of the ten speculative markets for which quantity data is available (see below in Section 1.6.1.2 for the definition of speculative markets). On average, the speculative markets experienced more dramatic sales declines in 2010: Beijing (-36.1%), Shanghai (-42.3%), Shenzhen (-42.3%) and City 1 (-40.5%), though the other anonymous speculative market (City 2) experienced mild sales increase of 8.9%.

The changes in house price growth can be gauged by referring to Deng, Gyourko and Wu (2012), in which they estimate the Hedonic home price index for 35 Chinese cities. In their Figure 6 (Comparison of Real Newly-Built Housing Price Indexes), we can observe a kink in home price series (i.e., a sudden change in price appreciation rate) right after 2010Q1. This again, is consistent with the prediction that price appreciate rate would decline after the policy intervention in April 2010.

The fact that transaction volumes dropped in the short run and house price growth momentum slowed down is consistent with the predictions discussed in Section 1.5.1 and 1.5.2. Thus, “Document Number 10” seemed to be effective in taming housing demand and stabilizing house prices, both of which being the goals the central government plan to achieve.

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<sup>44</sup>In 2011, eighteen markets experienced sales declines and twelve had double-digit declines. Only ten markets experienced expansion in housing sales in both 2010 and 2011 and none of them are coastal markets. These ten markets are Changchun, Chongqing, Haerbin, Huhehaote, Jinan, Shenyang, Shijiazhuang, Wuhan, Xian and Xining.

## 1.6 Heterogeneous Policy Impact and Firm Attributes

So far, I have shown that “Document Number 10” was readily interpreted as a negative shock to housing demand and brought about substantial responses in the stock market. The stock responses, reflecting future fundamental changes in a timely manner, also carry important implications for the housing market.

Across firms, however, the intensity of market value decline depends on firm’s exposure to the housing policy, and thus reveals the value of firm characteristics as perceived by stock investors. In this section, I use this exogenous housing policy shift to explore the heterogeneity in policy impact across firms along three dimensions: firm concentration in non-residential properties, geographic exposure of residential properties and firm political connections.

### 1.6.1 Sources of Heterogeneity: Theory and Measurement

#### 1.6.1.1 Non-Residential Real Estate Concentration

The first relevant aspect of real estate firms I consider is what types of properties firms develop or manage. In this case, building or owning non-housing assets would buffer the negative shock because the part of firm value coming from non-housing business lines would be less affected by the policy intervention. To formalize this idea, suppose a portion  $\alpha$  of firm’s market value comes from its non-housing assets  $V_{\text{non-housing}}^* = \alpha V^*$ , and the rest comes from housing development  $V_{\text{housing}}^* = (1 - \alpha)V^*$ . Market value change for such a diversified real estate firm can be modified as:

$$\frac{\Delta V^*}{V^*} = \frac{\Delta V_{\text{housing}}^*}{V_{\text{housing}}^* + V_{\text{non-housing}}^*} = \frac{(1 - \alpha)\Delta V^*}{V^*}$$

Accordingly, a positive share ( $\alpha$ ) of non-housing business would buffer the decline

in firm value after the announcement of a housing-specific policy. One of the implicit assumptions here is that the non-housing business line is independent from housing development. It is a convenient assumption, but not necessary. However, as long as the non-housing business would not be hit by the housing policy as severely or directly as the housing business, the prediction would hold.

I collect information about property concentration from firm annual reports. I then double check with information disclosed on official websites of the firms. Unfortunately, the share of non-housing business is not available in percentage terms because most of the firms do not report detailed breakdown of their portfolio, such as the percentage of revenue coming from non-housing business. Instead, I construct a dummy variable indicating whether firms explicitly mention non-residential real estate (usually commercial real estate) as their “major line of business” in addition to housing.<sup>45</sup>

According to Panel B of Table 1.9, 97 (or 75%) of the 129 real estate firms are housing-oriented developers. This is consistent with the fact that the majority of Chinese real estate firms do not have much expertise in managing non-residential properties. The remaining 32 firms have non-residential real estate as their major business in addition to housing development.

### 1.6.1.2 Geographic Exposure of Residential Properties

#### *The Importance of Geographic Exposure*

To the extent that the housing policy may have differential impacts across markets, geographic exposure of the housing properties also matters. Here, I consider two mechanisms through which geography affects policy impact.

One mechanism involves the nature of local housing markets before the policy. In an extreme case where the market is composed merely of first-home buyers, house

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<sup>45</sup>To do that, I perform a textual search of firm annual reports. This information is usually reported in the company profile or in the section introducing major business lines.

price would not respond to changes in second-home mortgage policies. In more general cases, markets with more speculators are expected to experience larger drops in equilibrium prices and quantities all else equal.<sup>46</sup> Thus, developers in those markets would suffer more severe declines in firm value.

Another mechanism has to do with local government efficiency and incentives. First, the housing policy may be executed in some cities with a lag because of lower government efficiencies. Second, to the extent that local governments rely on land sales as a major source of revenue, more cash-constrained local governments may have weaker incentives to follow the orders of the central government. In both cases, the same housing policy may be implemented less effectively in those markets.

#### *Measures of Geographical Exposure*

Unfortunately, there are no publicly-available measures for the share of speculators or the effectiveness of housing policy enforcement across markets. Instead, I use confidential data on the number of distant home buyers for 80 Chinese cities from January 2009 to March 2010.<sup>47</sup> For each city, I calculate the percentage share of buyers from outside the province/municipality. The ten cities with the highest share of distant buyers are referred to as “speculative markets”. I then search the firm annual reports over time and construct a dummy variable called “speculative markets” to indicate whether each firm develops housing properties in at least one of the ten speculative markets.

The ten markets cover three municipalities and three coastal provinces. Besides attracting more distant buyers who are likely to be speculators, the three municipalities and at least one coastal province are known for their efficient local governments, and serve as the role models for other Chinese cities. Therefore, distinguishing the “speculative markets” from others would reasonably capture the two mechanisms of

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<sup>46</sup>This is based on the assumptions and discussions laid out in Section 1.5.1.

<sup>47</sup>Names of the cities and provinces are screened in this paper for confidential concerns.

geography exposure discussed above. According to Table 1.9, about 78% of the firms have residential exposure in speculative markets.

### 1.6.1.3 Political Connections

#### *Why Political Connections Matter for the Housing Policy Impact?*

In this context of the April housing policy, the direction in which political connections drive the change of firm value is ambiguous because there are two mechanisms operating in opposite directions.

On the one hand, it is widely recognized in China that political connections bring both pecuniary and non-pecuniary benefits to firms. Having political connections implies easier access to bank loans, licenses, other resources key to business operation (Bai et al., 2000, 2006; Li et al., 2008; Allen et al., 2011) as well as favorable treatment in legal disputes (Li et al., 2008; Lu et al., 2011). Although little empirical evidence is available, political connections may be equally valuable to real estate firms because real estate is a capital-intensive business and government bureaus are involved in each stage of housing development. Extensive networking and rent-seeking are often necessary in acquiring financing, land, zoning approvals, and all kinds of construction permits. Therefore, having political connections may well save financing costs and operational costs of real estate firms. These durable benefits would help mitigate the impact of negative housing policy shocks and help firms in distress as well. For example, when market conditions become worse and firms are not able to sell enough units to cover their financing and construction costs, connected firms have easier access to loans provided by state banks, while private firms have to rely on their own resources.

On the other hand, politically-connected firms are more sensitive to government attitudes toward the real estate market. In other words, political connections could amplify the impact of the housing policy on more connected firms. This can be

explained by the following facts. First, the majority of the government-sponsored support in the 2008-2009 stimulus plans was granted to politically connected firms.<sup>48</sup> The new housing policy, however, diverges from those previous policies and signals a withdrawal of government support from the housing market. As such, one might expect politically connected firms to experience a more severe loss from this negative policy turn. Second, politically connected firms, supervised by the governments, bear the mission of carrying out government policies.<sup>49</sup> Entrepreneurs from those firms also have strong incentives to obey the rules because they may become politicians later in their careers. In addition, politically connected firms may also undertake more social responsibilities and engage themselves in less lucrative business such as constructing government-sponsored housing.<sup>50</sup> In sum, this housing policy shift initiated by the central government may disproportionately hurt politically connected firms, especially those connected to the central government.

### *Measures of the Degree of Political Connections*

The political economy literature provides a multitude of measures for political connections. In democratic countries, the CEOs' or board members' personal connections with a top politician are most commonly used to measure political connectedness (Faccio 2006; Goldman et al., 2009; Acemoglu et al., 2010). Less durable connections based on campaign contributions or cash transfers are also proposed (Claessens et al., 2008). However, these measures are not available in China because personal linkages between government officials and entrepreneurs/firms are either unobserved or not reputably documented.

In this paper, I make use of the state ownership status of the largest shareholder

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<sup>48</sup>For example, Deng, Morck, Wu and Yeung (2011) find that nearly 60% of all new loans issued to listed companies in the two mainland stock exchanges were received by state-owned enterprises affiliated with the central government during the stimulus period.

<sup>49</sup>Also see the “multi-task theory” of state-owned enterprises in China (Bai et al., 2000, 2006).

<sup>50</sup>As briefly mentioned in Article #7 of “Document Number 10”, politically connected firms are called upon to participate in the construction of government-sponsored housing units, which is less profitable than regular business of developing commodity housing units.



of each firm. It is a direct and objective indicator for firms’ political connectedness in China. Specifically, I define a real estate firm as a state-controlled firm if it or its largest shareholder is a state-owned enterprise (SOE) or a government entity.<sup>51</sup> Firms that do not belong to this category are defined as private firms, which by default have lower degrees of political connections than their state-controlled counterparts.<sup>52</sup>

Further, I distinguish two kinds of state-controlled firms based on the types of affiliation of their largest shareholders. “Central state-controlled” refers to state-controlled firms whose largest shareholders are affiliated with the central government, and “local state-controlled” refers to those whose largest shareholders are affiliated with local governments. It is worth noting that these two types of firms have different types of political connections.

In general, central state-controlled firms are believed to have broader and possibly stronger connections than local state-controlled ones. However, in a localized business such as real estate development, local state-controlled firms may reap extra benefit from their acquaintance with local bureaus and administrations. The central vs. local distinction is even more important here because the policy intervention was announced by the central government. Naturally, central state-controlled firms would be affected more directly and severely according to the second channel of political connections discussed above. Local governments, on the other hand, are required to but are less incentivized to carry out the new policy. In this sense, connections with local governments tend to be more “beneficial” to firms than connections with the central government.

Besides distinguishing the three types of firms, I also experiment with a contin-

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<sup>51</sup>This information is obtained from firm annual reports where the top ten shareholders are reported.

<sup>52</sup>It is worth noting that this paper identifies more state-controlled firms than the official list of state-owned enterprises (SOE) provided by the State-owned Assets Supervision and Administration Commission (SASAC) of the State Council. A broader definition of state-controlled firms is more appropriate in this case because it takes into account both subsidiary firms of the “official” SOEs and firms of which SOEs remain the largest shareholders and have control over.

uous measure of political connections. For each firm, I calculate the percentage of “state-controlled shares” held by its largest shareholder. By construction, it equals to the share of its largest shareholder if the firm is state-controlled, and zero if it is a private firm. This variable, however, has a drawback. Although state shares may suggest the intensity of political connections within each firm (Calomiris, Fisman and Wang, 2010), this is not necessarily true across firms. Namely, the degree of political connections may not be monotonically increasing in state-controlled shares across firms. The categorical measure of political connections based on firm type is cleaner and more appropriate and is thus taken as the baseline measure in this paper. Later, I report the results using the continuous measure of state-controlled shares as a robustness check.

According to Table 1.9, 55 (or 43%) out of the 129 real estate companies in the final sample are state-controlled, of which 14 are affiliated with the central government. Panel A of Table 1.9 summarizes the financial characteristics of firms by degree of political connections. Not surprisingly, central state-controlled real estate firms are significantly larger by market capitalization than private firms and local state-controlled firms. They also have stronger balance sheets than local state-controlled firms in terms of leverage, though not statistically distinguishable from private firms at conventional significance levels.

### 1.6.2 Regression Framework

Now, I take the above intuitions and examine the relationship between abnormal returns and real estate firm traits in a regression framework. I estimate the following equation:

$$CAR_i = \alpha + \beta \text{NonResidential}_i + \delta \text{SpeculativeMarket}_i + \eta \text{Connection}'_i + Z'_i \rho + \xi_i \quad (1.3)$$

where  $CAR_i$  is the estimated cumulative abnormal return for firm  $i$  over a certain event window;  $NonResidential_i$  is an indicator of whether firm  $i$  has a concentration in non-residential real estate management or development;  $SpeculativeMarket_i$  indicates whether firm  $i$  develops residential properties in at least one of the ten speculative markets; and  $Connection'_i$  is a vector of dummies indicating firms with various degrees of political connections (central state-controlled firms, local state-controlled firms and private firms). In addition, a set of firm-level covariates are included in  $Z'_i$ .

Equation (1.3) is adapted from the empirical specifications in the political economy literature that use instantaneous stock market responses to recover the value of firm political connections.<sup>53</sup> Abnormal stock returns around an exogenous event are regressed on the variables indicating degrees of political connections, while controlling for other basic firm characteristics. The literature varies in the choice of controls. One common practice is to not control for any other firm traits besides the direct variables of interest (for example, Fisman, 2001; Fisman et al., 2006). Others consider basic controls such as firm size, leverage and book-to-market ratio (see Goldman et al., 2009; Acemoglu et al., 2010). In this paper, I control for firm size (represented by the natural logarithm of market capitalization), leverage (measured by the ratio of net debt to common equity of shareholders) and firm profitability (proxied by return on equity) at the end of 2009.

Regarding the dependent variable, there is no standard in the literature for the length of event window over which to calculate CARs. Here, I follow the convention by considering an event window that ends shortly after the policy event. I then report results over longer event windows for comparison.<sup>54</sup> Given the complexity

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<sup>53</sup>For example, Fisman (2001), Fisman et al. (2006), Faccio (2006), Faccio et al. (2006), Acemoglu (2010), etc. This line of research collects surprising news about politicians, compares news-induced abnormal stock returns across firms with various degrees of connections to those politicians and thereby recovers the value of firm political connections.

<sup>54</sup>For example, Fisman (2001) and Fisman et al. (2006) use market-adjusted stock returns over one day or five days after the event. Faccio (2006) uses  $CAR[-2, 2]$  and Goldman et al. (2008) use  $CAR[1, 3]$ . Acemoglu et al. (2010) uses  $CAR[0, 1]$  in the baseline result and then  $CAR[0, 10]$  as a robustness check.

that the housing policy event consists of pre-event notifications as well as post-event announcement, I use  $CAR_i[-20, 5]$  for firm  $i$  as the dependent variable in the benchmark specification. As discussed in Section 1.4.3, the  $[-20, 5]$  window is believed to capture the full impact of the housing policy shift. It also reasonably represents the “immediate” response without introducing longer-run noise. I then experiment with CARs over various horizons in 1.6.4.2 and 1.6.4.3 as robustness checks.

### 1.6.3 Identification

Before moving on to the result, I talk about identification for the three firm attributes of interest in the above framework. As argued in Section 1.4.1, the policy intervention is exogenous to the value of real estate firms. What remains an empirical challenge is the possibility of omitted factors biasing the result.

While the existing literature provides little theory or empirical strategies to deal with the omitted variable bias, I address this concern first by adopting an extensive list of control variables in  $Z'_i$ , which embrace firm size, leverage and profitability. These characteristics may well affect the abnormal return of real estate firms in response to the negative housing policy shock. For example, more profitable firms may have better management teams and business strategies to get through adverse market conditions. Firms overloaded with debt would be more vulnerable to market downturns. As those basic firm characteristics are likely to be correlated with the variables of interest (especially firm political connections as we can tell from Table 1.9), ignoring them could bias the estimates of interest.

Second, I argue that some hypothesized concerns are not likely to pose threats here because of some unique features of the Chinese housing market. By and large, one would be worried that some omitted factors may potentially lead to differential housing policy impact, but not through the channels of the firm characteristics of interest. For example, if more speculators buy homes from state-controlled developers,

then the estimate for political connections would reflect the fundamental demand shock which hit those firms more severely, rather than the true value of political connections. Although this concern is legitimate, it is not likely to be the case in China because state-controlled firms do not intentionally attract or look for more speculative clientele. Only banks are able to know whether mortgage applicants are speculators or not.<sup>55</sup> Moreover, real estate firms in China, regardless of political connections or other attributes, develop more or less similar housing units in terms of location and physical attributes. In this regard, it is fair to assume that the speculator-taming demand shock hit firms in similar manners, and thus the differential policy impacts across firms should come from the firm attributes of interest.

Finally, I explicitly consider one particular kind of omitted variable. Since the housing policy implies a turning point in the housing market, one natural possibility is that real estate firms which benefited the most during the previous stimuli would be subject to the strongest doubts over their growth potential. It is not clear why some real estate firms enjoyed higher growth than others during the incredible boom, but unobserved factors may have played an important role.<sup>56</sup> These unobserved characteristics may drive firms' abnormal loss in a potential downturn as well beyond observed attributes. To address this possibility, I include the growth rate of firms from 2008 to 2009 in terms of market value and book value as additional controls in the model.

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<sup>55</sup>Technically, only banks are able to distinguish speculators from first-home buyers when they conduct background checks on mortgage applicants. Banks also are responsible for imposing the differential leverage requirements on each type of buyers.

<sup>56</sup>This argument is supported by the fact that the unprecedented growth in the housing market during 2008 and 2009 can hardly be justified by fundamentals (Wu, Gyourko and Deng, 2012).

## 1.6.4 Result

### 1.6.4.1 Benchmark Result

Table 1.10 reports the baseline results of estimating equation (1.3) using cumulative abnormal returns from day -20 to day 5 ( $CAR[-20, 5]$ ) as the dependent variable. Bootstrapped standard errors are reported. Stock exchange dummies are included in all specifications to capture common factors pertaining to the stock exchanges. Columns (1) and (2) represent the unconditional effects coming from the firm traits of interest – non-residential real estate concentration, geographic exposure of housing properties and degree of political connections, which alone explain more than 17% of the variation in abnormal stock returns. The preferred benchmark specification in column (3) controls for all other firm covariates, such as firm size, leverage and profitability. This weakens but does not wipe out the significance of the estimates of interest.

As expected, the coefficient on non-residential real estate is positive and significant. Namely, a concentration in non-residential properties brings a higher abnormal return of 3%, buffering the negative shock from the housing policy. Residential exposure in speculative markets causes more negative abnormal returns but the effect is barely significant in a statistical sense. However, this should not be interpreted as geography not mattering. In fact, the economic importance of geographic exposure is likely to be obscured by the fact that quite a few real estate firms have been diversifying their housing properties across markets. For example, more than 50% of the firms in the sample operate in more than three provinces/municipalities, and most of them have been expanding their business to provincial cities and second-tier or third-tier cities rather than confining themselves to first-tier cities.

The coefficient on the central state-controlled firms is -0.054 and statistically significant, suggesting that firms controlled by the central government or other central

state-owned enterprises suffer an additional abnormal return of -5.4% compared to private real estate firms over the  $[-20, 5]$  event window. The response of local state-controlled firms is not distinguishable from that of private firms. It is also worth noting that the relative ranking of coefficients on political connections is robust to the inclusion of other firm-level characteristics. On average, central state-controlled firms suffer the most severe losses and private firms the least.

The negative coefficients on state-controlled firms suggest that the second channel of political connections (as discussed in Section 1.6.1.3) plays a dominant role in this case. Namely, the expectation that state-controlled firms would suffer disproportionately from a reduction in government support and that they would follow government policies more actively, outweighs the widely-acknowledged benefits of political connections in buffering negative market shocks. In addition, the second channel is assumed to affect firms affiliated with the central government more directly because the housing policy is decided by the central government. This is born out in the empirical results where state-controlled firms affiliated to the central government experience more negative abnormal returns than those affiliated with local governments.

Column (4) uses the percentage of state-controlled shares held by the largest shareholder as a proxy for firm political connections. I also include its interaction term with being a central state-controlled firm to gauge the difference between connections with the central government versus local governments. In spite of its flaw in reflecting true variations in political connections across firms (as discussed in Section 6.1.3), this continuous state-share variable predicts more negative abnormal returns for state-controlled firms, especially for those connected with the central government.<sup>57</sup> However, this effect is not statistically significant at conventional levels.

Coefficients on other firm-level covariates are suppressed in the table. Only firm

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<sup>57</sup>To interpret the coefficient, a one percentage point increase in the state shares held by the largest shareholder would lead to an additional -10 basis points abnormal return for a central state-controlled firm.

size has a significant impact. A 1% increase in market capitalization leads to an additional abnormal return of around -1.5 basis points. In case of outliers driving the result, column (5) repeats the same regression as column (3) but on a restricted sample where firms in the top and bottom 2% distribution of  $CAR[-20, 5]$  are trimmed. This produces similar estimates as the full sample.

Following discussions in Section 1.6.3, I then capture omitted factors by controlling for firms' growth rate in terms of market capitalization and total asset (at book value) from 2008 to 2009. Estimates are reported in column (2) and (3) of Table 1.11, which shift the baseline estimates in column (1) only slightly. Firm concentration in non-residential business and political connections continue to matter beyond omitted factors. In addition, the coefficient on the 2008-to-2009 growth rate of total assets is significantly negative, suggesting that all else equal, an additional 1% increase in the book value of total asset during the boom is associated with an additional cumulative abnormal return of -4.2 basis points.

#### 1.6.4.2 Robustness Tests: Alternative Event Windows

In order to confirm that the above results reflect the true relationship between abnormal policy response and firm traits of interest rather than some spurious local correlations in data, I estimate equation (1.3) using cumulative abnormal returns over alternative event windows as the dependent variables in Table 1.12.

Column (1) is the benchmark result (taken from column (3) in Table 1.10) for comparison. Columns (2) to (3) use more immediate abnormal returns after the release of the official policy document,  $CAR[-20, 3]$  and  $CAR[-20, 4]$ , respectively as dependent variables. Both of them produce similar results to the benchmark. Firms with non-residential real estate concentrations suffer less compared to housing-oriented developers. Similarly, central state-controlled firms exhibit significantly more negative abnormal returns than private firms, and the discrepancy is bigger when we



look at more instantaneous responses. Column (4) examines longer-run abnormal return until day 10 and the relationship is more or less similar to that in the short run. The benefit of engaging in non-residential real estate seems to accumulate 10 days after the policy announcement. The last column uses alternative starts of event windows and examines  $CAR[-10, 5]$ . Again, the estimates stay similar to those in the benchmark specification.

#### 1.6.4.3 Result over the Course of Policy Release

The next question of interest involves how the relationship between abnormal returns and firm traits changes over the course of the policy announcement. To investigate this, I estimate the baseline specification sequentially using  $CAR[-20, k]$  ( $k \in [-20, 20], k \in Z$ ) as the dependent variables. Figure 1.10 and 1.11 plots the point estimates and the 95% confidence intervals over  $k$  for the four coefficients of interest. The figures are truncated at  $k = 10$  because this paper focuses on short-run stock responses. Relationships and abnormal returns over the longer run are subject to noise.

Figure 1.10 plot coefficients on non-residential real estate concentration and speculative markets, respectively. Again, property diversification is an important determinant of abnormal response to the housing policy shift. Compared with housing-oriented developers, firms with non-housing concentration enjoy an additional CAR of 2% to 3%, mitigating the negative policy shock. Residential exposure in superstar markets, however, does not matter significantly in statistical sense. As is discussed above, those firms have already diversified their portfolios geographically. Figure 1.11 plot the two coefficients on state-controlled firms. Central state-controlled firms suffer an additional CAR of -5% to -7% compared to private firms during the course of the policy issuance. The coefficient on local state-controlled firms, however, is not significantly distinguishable from zero.

One noteworthy finding is that firm traits begin to matter only after we observe abnormal returns. This is consistent with the intuition that the relationship between abnormal returns and firm traits should appear only when the abnormal returns show up. During normal periods, there is little justification for such a relationship. The effect of political connections synchronizes the time line of the policy announcement, which does not become significant until people learned of the BFA speech on mortgage regulations (after day -4). The value of non-residential real estate concentration, however, is not revealed until the full release of the official policy document (after day 1). This timing difference can be explained by how the policy content was gradually released. Before day 2, only the core piece of “Document Number 10” – tightening mortgage requirements for speculators – was unveiled to the public. Only after reading the entire policy document did investors learn that the policy was only targeted at the housing market, thus allowing them to form differential beliefs about firms with non-residential real estate concentrations.

## 1.7 Conclusion

In this paper, I use high frequency financial market data and the event study approach to provide the first estimates of the impact of the Chinese housing policy reversal in April 2010, which tightened mortgage credit supply for housing speculators. The policy brought about timely and economically meaningful responses in the stock market. It produced cumulative abnormal returns for affected real estate firms of about -15%, which is consistent with an up to 10 percentage point drop in equilibrium house price appreciation rate.

Across firms, I find that some concentration in non-residential real estate provides a hedge against housing-specific policy risks, mitigating the negative abnormal return by about 3 percentage points. Meanwhile, firms whose largest shareholder is

a state-owned enterprise affiliated with the central government suffer 5 percentage points more abnormal returns compared to observationally equivalent private firms. In this case, the conventional benefits of political connections are counteracted by the expectation that connected firms would be hurt more severely from a withdrawal of government support and that they would follow central policies more actively than other firms.

This paper is the first to use equity market data to evaluate the impact of specific housing policy changes by the Chinese government. The event study methodology is shown to be appropriate in this Chinese policy context, and is useful especially when appropriate data on the underlying housing market are not readily available. Policy makers would thus be able to gauge immediately the effectiveness of policies and predict expected changes in the housing market and in other related industries, such as the steel and construction industry. The approach also is insightful in that abnormal stock returns provide meaningful implications for the values of firm attributes that are particularly relevant to real estate firms. More generally, the success of this research design in evaluating the April 2010 policy reversal suggests that the equity markets could prove very useful in analyzing the consequences of other housing-related events and the effectiveness of policy interventions in other sectors where primary data quality is flawed.

This paper also contributes to the understanding of housing developers in China. For example, it is worthwhile for firms to diversify their property portfolio because engaging in non-residential development may provide a hedge against housing policy risks. The differential policy impact on state-controlled firms and private firms suggests some role of political connections in the housing market. However, to fully understand and distinguish the underlying mechanisms would require more sophisticated models and explicit measures of the benefits and costs that are tied with political connections.

Within the housing literature, this paper provides novel empirical evidence of the impact of tightening down payment requirements on housing demand and housing prices. While research in this field has been challenged by the endogeneity of leverage decisions, I investigate an exogenous change in leverage requirements in China and provide reduced-form estimates. I document an up to 10 percentage point drop in equilibrium house price growth rate associated with a 10 percentage point increase in required minimum down payments for housing speculators. This estimate tends to be useful for us to interpret consequences of leverage policies in other countries as well. However, one should be careful generalizing predictions in this paper to a broader context because of some unique characteristics of the Chinese mortgage market and mortgage policies.

Finally, one of the limitations of this paper and this method is that the results represent only the perceived value of real estate firms around the policy announcement, but do not perfectly predict the performance of the housing market or real estate firms in the long run. Future work remains to be done to fully understand the resulting impact of mortgage policies on housing demand, house prices and so on. More interesting topics, such as how home investors, especially those near the down payment thresholds respond to the changes in leverage regulations and how the impact of the mortgage policy varies across markets, will require different research designs as well as high-quality housing transaction data.

Table 1.1: Publicly Traded Real Estate Firms as a Percentage of All Real Estate Firms

Year	Share of Publicly Traded Real Estate Companies		
	Number of Firms	Total Asset	Sales Revenue
2008	0.15%	7.1%	13.4%
2009	0.16%	7.9%	14.0%
2010	0.15%	8.4%	15.0%

Note: Information about publicly traded real estate firms is calculated based on the sample adopted in this paper (See Section 1.3 for the selection of this sample). Data on all real estate firms are available from the National Bureau of Statistics.

Table 1.2: Sample of Publicly Traded Real Estate Firms

Stock Market	(Market capitalization in billion USD)			
	All Publicly Traded Firms		Publicly Traded Real Estate Firms	
	# of Firms	Median Market Cap (Std. dev.)	# of Firms	Median Market Cap (Std. dev.)
HKEx	1126	0.191 (16.981)	46	0.909 (3.339)
SSE	743	0.738 (11.004)	45	1.071 (2.067)
SZSE	719	0.599 (1.857)	38	0.905 (2.988)

<sup>1</sup> Note: The left panel reports the number of firms publicly traded on the HKEx (main board), the SSE (A-share market) and the SZSE (main board and A-share market) up until December 31, 2009, together with the median and standard deviation of market capitalization at the end of 2009. The right panel reports the same statistics for the sample of publicly traded real estate firms that is adopted in this paper.

Table 1.3: Average Cumulative Abnormal Return (CAR) of Real Estate Firms

$\overline{CAR}[-20, k]$	All Real Estate Firms (1)	By Stock Exchange	
		HKEX (2)	Mainland (SSE & SZSE) (3)
[-20,-10]	0.000 (0.009)	-0.008 (0.018)	0.004 (0.009)
[-20,-6]	-0.014 (0.011)	-0.004 (0.022)	-0.019 (0.011)
[-20,-5]	-0.019 (0.011)	0.001 (0.023)	-0.031** (0.012)
[-20,-4]	-0.027* (0.011)	-0.007 (0.023)	-0.038*** (0.012)
[-20,-3]	-0.062*** (0.012)	-0.030 (0.024)	-0.080*** (0.012)
[-20,-2]	-0.060*** (0.012)	-0.046 (0.025)	-0.068*** (0.013)
[-20,-1]	-0.065*** (0.012)	0.051* (0.025)	-0.072*** (0.013)
[-20, 0]	-0.070*** (0.013)	-0.056* (0.026)	-0.078*** (0.013)
[-20,1]	-0.076*** (0.013)	-0.065** (0.026)	-0.083*** (0.014)
[-20,2]	-0.100*** (0.013)	-0.078*** (0.027)	-0.113*** (0.014)
[-20,3]	-0.141*** (0.013)	-0.110*** (0.028)	-0.159*** (0.014)
[-20,4]	-0.143*** (0.014)	-0.111*** (0.028)	-0.160*** (0.014)
[-20,5]	-0.155*** (0.014)	-0.104*** (0.029)	-0.183*** (0.015)
[-20,10]	-0.131*** (0.015)	-0.124*** (0.031)	-0.135*** (0.016)
N	129	46	83

<sup>1</sup> Note: Average cumulative abnormal returns over event windows  $[-20, k]$  and their asymptotic standard errors are reported in cells. For space reasons, only selected  $k$ 's are displayed. The complete schedule of  $\overline{CAR}[-20, k]$  is plotted in Figure 1.4. I test the null hypothesis of no abnormal return among real estate firms on average. The null hypothesis being rejected at significance levels 5%, 2% and 1% is denoted by \*, \*\* and \*\*\*, respectively.

Table 1.4: Average CAR of Real Estate Firms: Alternative Event Windows

Event Window: $[-20, k]$		Event Window: $[-10, k]$		Event Window: $[-5, k]$	
$\overline{CAR}[-20, k]$		$\overline{CAR}[-10, k]$		$\overline{CAR}[-5, k]$	
[-20,-10]	0.000 (0.009)	—		—	
[-20,-7]	-0.009 (0.010)	[-10,-7]	-0.007 (0.006)	—	
[-20,-6]	-0.014 (0.011)	[-10,-6]	-0.012 (0.007)	—	
[-20,-5]	-0.019 (0.011)	[-10,-5]	-0.016* (0.007)	[-5,-5]	-0.005 (0.003)
[-20,-4]	-0.027* (0.011)	[-10,-4]	-0.025*** (0.008)	[-5,-4]	-0.013*** (0.004)
[-20,-3]	-0.062*** (0.012)	[-10,-3]	-0.061*** (0.008)	[-5, -3]	-0.046*** (0.005)
[-20,-2]	-0.060*** (0.012)	[-10,-2]	-0.060*** (0.008)	[-5,-2]	-0.044*** (0.005)
[-20,-1]	-0.065*** (0.012)	[-10,-1]	-0.064*** (0.009)	[-5,-1]	-0.048*** (0.006)
[-20, 0]	-0.070*** (0.013)	[-10,0]	-0.070*** (0.009)	[-5,0]	-0.053*** (0.006)
[-20,1]	-0.076*** (0.013)	[-10,1]	-0.075*** (0.010)	[-5,1]	-0.058*** (0.007)
[-20,2]	-0.100*** (0.013)	[-10,2]	-0.099*** (0.010)	[-5,2]	-0.081*** (0.007)
[-20,3]	-0.141*** (0.013)	[-10,3]	-0.139*** (0.010)	[-5,3]	-0.120*** (0.008)
[-20,4]	-0.143*** (0.014)	[-10,4]	-0.140*** (0.011)	[-5,4]	-0.121*** (0.008)
[-20,5]	-0.155*** (0.014)	[-10,5]	-0.151*** (0.011)	[-5,5]	-0.133*** (0.009)
[-20,10]	-0.131*** (0.015)	[-10,10]	-0.128*** (0.013)	—	
N	129	N	129	N	129

<sup>1</sup> Note: This table compares average cumulative abnormal returns of the 129 real estate firms calculated using various event windows:  $[-20, k]$ ,  $[-10, k]$  and  $[-5, k]$ . The null hypothesis being rejected at significance levels 5%, 2% and 1% is denoted by \*, \*\* and \*\*\*, respectively.



Table 1.5: Average CAR of Real Estate Firms: Time-Shifted Placebo Events

Backward Placebo Events		Forward Placebo Events	
Placebo Dates	$\overline{CAR}[-2, 2]$	Placebo Dates	$\overline{CAR}[-2, 2]$
Day -20	0.008 (0.006)	Day 20	0.000 (0.006)
Day -30	0.008 (0.006)	Day 30	-0.001 (0.006)
Day -40	0.005 (0.008)	Day 40	-0.013* (0.006)
Day -50	0.006 (0.006)	Day 50	-0.013* (0.006)
Day -100	-0.003 (0.006)	Day 100	-0.005 (0.005)
Day -150	0.007 (0.007)	Day 150	-0.006 (0.006)
N	129	N	129

<sup>1</sup> Note: Average 5-day cumulative abnormal returns around placebo events and the asymptotic standard errors are reported. Placebo event dates are represented by the relative trading day to the announcement of the April policy intervention. The null hypothesis being rejected at significance levels 5%, 2% and 1% is denoted by \*, \*\* and \*\*\*, respectively.

Table 1.6: Average Cumulative Abnormal Return of Non-Real-Estate Firms Publicly Traded on HKEx

$\overline{CAR}[-20, k]$	Industrial (1)	Textile (2)	Food&Beverage (3)	Materials (4)	Mining (5)	Communication (6)	Service (7)	Energy (8)
[-20,-10]	0.017 (0.015)	-0.003 (0.019)	0.001 (0.019)	-0.005 (0.021)	-0.028 (0.039)	0.041 (0.035)	-0.013 (0.021)	-0.021 (0.022)
[-20,-5]	0.025 (0.019)	-0.005 (0.025)	-0.021 (0.024)	0.011 (0.029)	0.011 (0.051)	0.047 (0.046)	-0.013 (0.028)	-0.005 (0.028)
[-20,-2]	0.021 (0.021)	-0.009 (0.027)	-0.032 (0.026)	0.002 (0.031)	0.024 (0.055)	0.035 (0.050)	-0.039 (0.030)	-0.013 (0.030)
[-20, 0]	0.032 (0.022)	0.002 (0.028)	-0.030 (0.028)	-0.007 (0.033)	0.027 (0.058)	0.048 (0.052)	-0.044 (0.031)	-0.021 (0.032)
[-20,2]	0.034 (0.023)	-0.003 (0.030)	-0.032 (0.029)	-0.016 (0.034)	0.021 (0.060)	0.026 (0.054)	-0.061 (0.033)	-0.009 (0.033)
[-20,5]	0.039 (0.024)	0.007 (0.031)	-0.027 (0.031)	-0.005 (0.036)	0.027 (0.064)	0.042 (0.058)	-0.052 (0.035)	-0.003 (0.036)
[-20,10]	0.044 (0.026)	0.007 (0.034)	-0.039 (0.034)	-0.022 (0.040)	0.020 (0.070)	0.058 (0.063)	-0.073 (0.038)	0.008 (0.039)
N	121	64	47	53	19	14	32	51

<sup>1</sup> The placebo analysis employs companies in eight randomly-selected industries according to the Hang Seng Industry Classification.

<sup>2</sup> Average cumulative abnormal returns over event windows  $[-20, k]$  and their asymptotic errors are reported at a 5-day interval. The null hypothesis being rejected at significance levels 5%, 2% and 1% is denoted by \*, \*\* and \*\*\*, respectively.

Table 1.7: Average Cumulative Abnormal Return of Non-Real Estate Firms Publicly Traded on the SSE and SZSE

$\overline{CAR}[-20, k]$	Agriculture (1)	Mining (2)	Manufacturing			Utilities (6)	Social Services (7)	Communication (8)
			Textile (3)	Petrochemicals (4)	Machinery (5)			
[-20,-10]	-0.043*** (0.014)	0.007 (0.012)	-0.004 (0.010)	0.004 (0.007)	0.001 (0.001)	-0.012 (0.009)	0.015 (0.011)	0.011 (0.020)
[-20,-5]	-0.033 (0.017)	0.020 (0.015)	-0.008 (0.012)	0.005 (0.009)	0.011 (0.011)	-0.007 (0.011)	0.010 (0.014)	0.017 (0.025)
[-20,-2]	-0.043* (0.019)	0.028 (0.016)	-0.004 (0.013)	-0.013 (0.011)	0.012 (0.012)	-0.013 (0.012)	0.008 (0.015)	0.023 (0.027)
[-20, 0]	-0.016 (0.020)	0.038* (0.017)	0.000 (0.014)	-0.009 (0.011)	0.017 (0.017)	-0.009 (0.012)	0.007 (0.016)	0.014 (0.029)
[-20,2]	-0.011 (0.021)	0.017 (0.018)	0.004 (0.014)	-0.014 (0.013)	0.021 (0.021)	-0.008 (0.013)	0.015 (0.017)	0.005 (0.030)
[-20,5]	0.020 (0.022)	0.019 (0.019)	0.011 (0.015)	-0.006 (0.013)	0.034 (0.034)	-0.007 (0.014)	0.033 (0.018)	0.039 (0.032)
[-20,10]	0.042 (0.024)	0.049* (0.022)	-0.006 (0.019)	-0.017 (0.014)	0.032 (0.032)	0.000 (0.016)	0.042* (0.019)	0.047 (0.036)
N	30	51	58	111	52	69	46	15

<sup>1</sup> The placebo analysis employs companies in eight randomly-chosen industries. According to the CSRC (China Security Regulatory Commission) Industry Classification rules, these eight industries are Agriculture, Forestry, Livestock Farming, Fishery (A), Mining (B), Textile and Apparel (C1), Petrochemicals (C4), Common Machines Manufacturing (C71), Utilities (D), Social Services (K) and Communication (L).

<sup>2</sup> Average cumulative abnormal returns over event windows  $[-20, k]$  and their asymptotic errors are reported at a 5-day interval. The null hypothesis being rejected at significance levels 5%, 2% and 1% is denoted by \*, \*\* and \*\*\*, respectively.

Table 1.8: Floor Area Sold of Residential Properties, 35 Major Chinese Cities

Year	Mean	Std. Dev.	(in million square meters)		
			25 <sup>th</sup>	Median	75 <sup>th</sup>
			Percentile		Percentile
2000	2.42	2.73	0.66	1.90	2.90
2001	2.92	3.29	0.90	1.90	3.53
2002	3.50	3.96	1.06	2.30	3.75
2003	4.28	4.63	1.67	2.91	4.63
2004	5.07	6.59	1.89	3.09	5.09
2005	7.04	6.53	2.42	5.37	8.95
2006	7.22	5.96	3.17	5.83	9.09
2007	8.76	7.54	3.82	6.68	10.65
2008	6.55	5.33	3.26	5.17	8.02
2009	9.96	7.91	4.43	7.60	12.54
2010	9.38	7.36	4.46	7.86	12.10
2011	9.28	7.50	4.44	7.69	11.69
N	35	35	35	35	35

<sup>1</sup> Source: National Bureau of Statistics of China.

Table 1.9: Description of Publicly Traded Real Estate Firms

		By Firm Type		
	All Firms (1)	Private Firms (2)	Central State- Controlled (3)	Local State- Controlled (4)
<i>Panel A: Financial Metrics</i>				
Market Capitalization (in Billion Chinese Yuan)	13.00 (18.72)	9.16 (10.84)	39.60 (41.19)	10.86 (8.74)
Leverage	0.341 (.503)	0.356 (.552)	0.143 (.336)	0.382 (.449)
Return on Equity	0.146 (.151)	0.136 (.171)	0.142 (.105)	0.167 (.124)
<i>Panel B: Growth Rate (2008-2009)</i>				
Growth Rate of Market Capitalization	1.801 (1.699)	1.825 (1.937)	2.276 (2.235)	1.596 (.842)
Growth Rate of Total Asset	.333 (.563)	.29 (.396)	.42 (.298)	.379 (.831)
<i>Panel C: Property Exposure</i> (Number of Firms in cell)				
Residential-Oriented	97	53	11	33
With Non-Residential Concentration	32	21	3	8
<i>Panel D: Geographical Exposure</i> (Number of Firms in cell)				
Speculative Markets	101	55	12	34
Number of Firms	129	74	14	41

<sup>1</sup> Note: Financial metrics at the end of 2009 are summarized in Panel A. Panel B shows the appreciation of firm market value and book value during the 2008-2009 stimulus period. Non-residential real estate concentration and geographical exposure in the ten speculative markets are described in Panel C and D.

Table 1.10: Policy Impact and Firm Attributes: Baseline Estimates

	Dependent Variable: $CAR_i[-20, 5]$				
	(1)	(2)	(3)	(4)	(5)
Non-Residential Real Estate	0.042*** (0.015)	0.040*** (0.015)	0.030** (0.012)	0.030** (0.012)	0.026** (0.011)
Speculative Markets	-0.049 (0.031)	-0.043 (0.031)	-0.045 (0.029)	-0.047* (0.028)	-0.041* (0.024)
Central State-Controlled		-0.074*** (0.026)	-0.054* (0.029)		-0.049* (0.025)
Local State-Controlled		-0.022 (0.026)	-0.012 (0.024)		-0.030 (0.020)
State-Controlled Shares				-0.012 (0.049)	
State-controlled Shares $\times$ Central State-Controlled				-0.088 (0.065)	
Constant	Y	Y	Y	Y	Y
Stock market dummies	Y	Y	Y	Y	Y
Other firm-level Covariates	N	N	Y	Y	Y
Trimmed sample	N	N	N	N	Y
N	129	129	129	129	124
R-Squared	0.176	0.214	0.257	0.254	0.310

<sup>1</sup> Note: This table reports results from estimating Equation (1.3). CARs are calculated in Section 1.4.3. “Non-Residential Real Estate” and “Speculative Markets” are indicators of whether firms have concentrations in non-residential properties and whether firms develop housing properties in speculative markets. “Central State-Controlled” and “Local State-Controlled” are indicators of firms with various types of political connections. Private firm is the omitted category here. Column (3) represents the baseline specification where the full set of other firm-level covariates (firm size, leverage, profitability) are included. Column (4) employs a continuous measure of political connections – “State-Controlled Shares”, which equals to the % share of the largest shareholder if the firm is state-controlled, and 0 if the firm is privately owned. The interaction of “State-Controlled Shares” and being a central state-controlled firm is also included in column (4). Column (5) is the same regression as column (3) but done on a trimmed sample where firms with the top and bottom 2%  $CAR[-20, 5]$  are dropped. Bootstrapped standard errors are reported. Significant levels 10%, 5% and 1% are indicated by \*, \*\* and \*\*\*, respectively.

Table 1.11: Policy Impact and Firm Attributes: Omitted Variable Bias

	Dependent Variable: $CAR_i[-20, 5]$		
	(1)	(2)	(3)
Non-Residential Real Estate	0.030** (0.012)	0.025** (0.013)	0.031** (0.012)
Speculative Markets	-0.045 (0.029)	-0.056* (0.029)	-0.052* (0.027)
Central State-Controlled	-0.054* (0.029)	-0.050* (0.027)	-0.049* (0.028)
Local State-Controlled	-0.012 (0.024)	-0.009 (0.025)	-0.009 (0.024)
Growth of Market Cap (2008-2009)		-0.011 (0.011)	
Growth of Total Asset (2008-2009)			-0.042** (0.017)
Constant	Y	Y	Y
Stock market dummies	Y	Y	Y
Other firm-level Covariates	Y	Y	Y
N	129	126	129
R-Squared	0.257	0.311	0.305

<sup>1</sup> Note: This table reports results from estimating Equation (1.3). Column (1) is the baseline specification as Column (3) of Table 1.10. Column (2) controls for the growth rate of firm market capitalization from 2008 to 2009. Column (3) controls for the growth rate of firm total asset during the same period. Bootstrapped standard errors are reported. Significant levels 10%, 5% and 1% are indicated by \*, \*\* and \*\*\*, respectively.

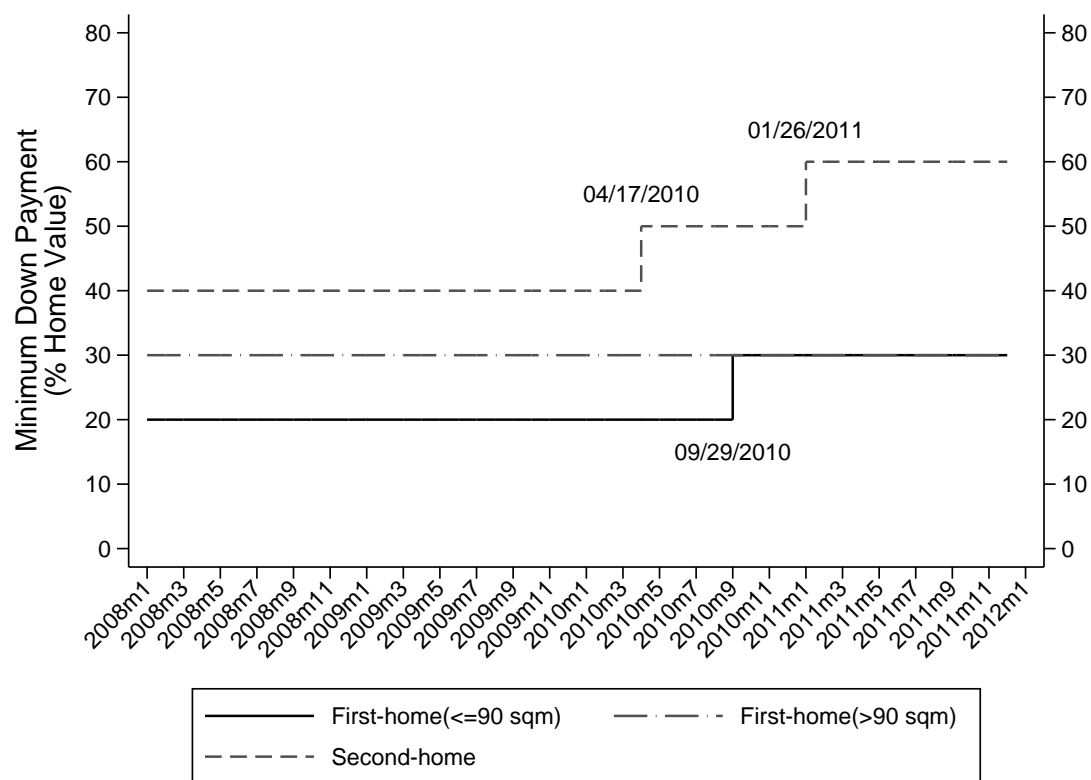
Table 1.12: Policy Impact and Firm Attributes: Alternative Event Windows

	<i>CAR</i> over Various Event Windows				
	[-20,5] (1)	[-20,3] (2)	[-20,4] (3)	[-20,10] (4)	[-10,5] (5)
Non-Residential Real Estate	0.030** (0.012)	0.021** (0.010)	0.030*** (0.011)	0.056** (0.023)	0.025** (0.011)
Speculative Markets	-0.045 (0.029)	-0.026 (0.025)	-0.031 (0.026)	-0.038 (0.031)	-0.027 (0.023)
Central State-Controlled	-0.054* (0.028)	-0.069** (0.028)	-0.061** (0.025)	-0.053* (0.031)	-0.045* (0.027)
Local State-Controlled	-0.012 (0.023)	-0.021 (0.021)	-0.015 (0.022)	-0.009 (0.028)	-0.008 (0.020)
Constant	Y	Y	Y	Y	Y
Stock market dummies	Y	Y	Y	Y	Y
Other firm-level Co-variates	Y	Y	Y	Y	Y
N	129	125	129	127	129
R-squared	0.268	0.273	0.235	0.218	0.325

<sup>1</sup> Note: This table reports estimation of the benchmark specification using cumulative abnormal returns calculated over various event windows as the dependent variables. Column (1) is the same regression as column (3) of Table 1.10. Bootstrapped standard errors are reported. Significant levels 10%, 5% and 1% are indicated by \*, \*\* and \*\*\*, respectively.



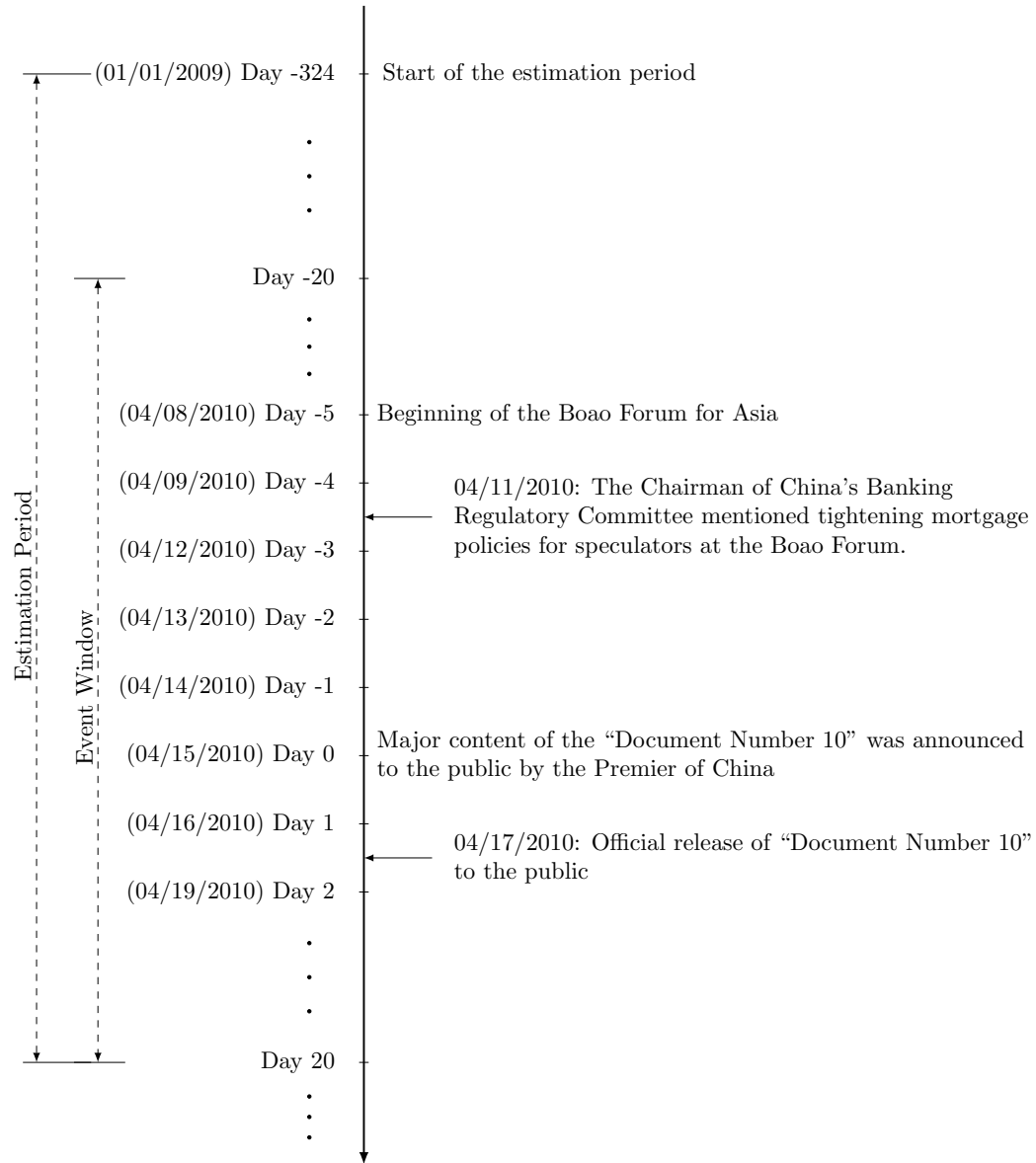
Figure 1.1: Minimum Down Payment Requirement



[1] The three discrete changes in minimum down payment rules represent the three housing policy interventions in April 2010, September 2010 and January 2011.

[2] Minimum down payment requirement for a third-home purchase (not plotted here) is greater than or equal to that for a second-home purchase. The September 2010 policy required banks to suspend loans for third-home purchases.

Figure 1.2: Time Line of the Announcement of “Document Number 10”



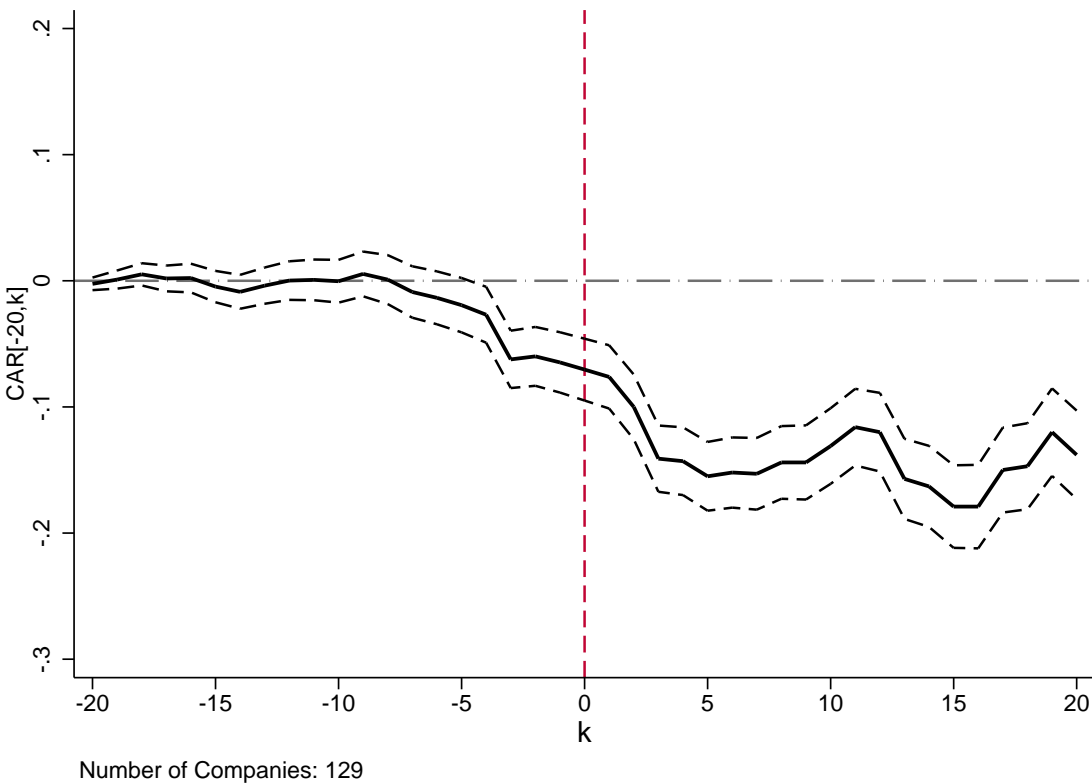
Note: The time line of information disclosure is collected from the Xinhua News Agency ([www.xinhua.org](http://www.xinhua.org)), which is the most authoritative media channel in China through which major news is released to the public.

Figure 1.3: The Real Estate Climate Index of China



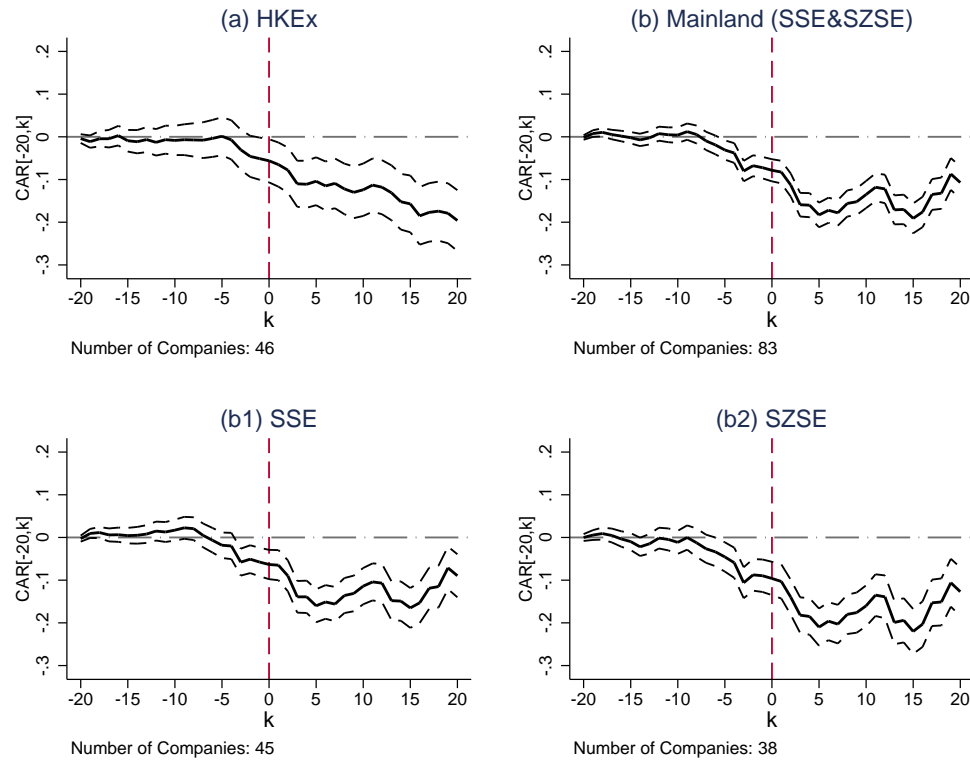
Note: This index is available from the National Bureau of Statistics, calculated on a monthly basis from real estate investment, development and sales data. A reading above 100 indicates good conditions, while one below that level indicates bad conditions. The vertical line represents March 2010 (right before the announcement of “Document Number 10”).

Figure 1.4: Average Cumulative Abnormal Return (CAR) of Real Estate Firms



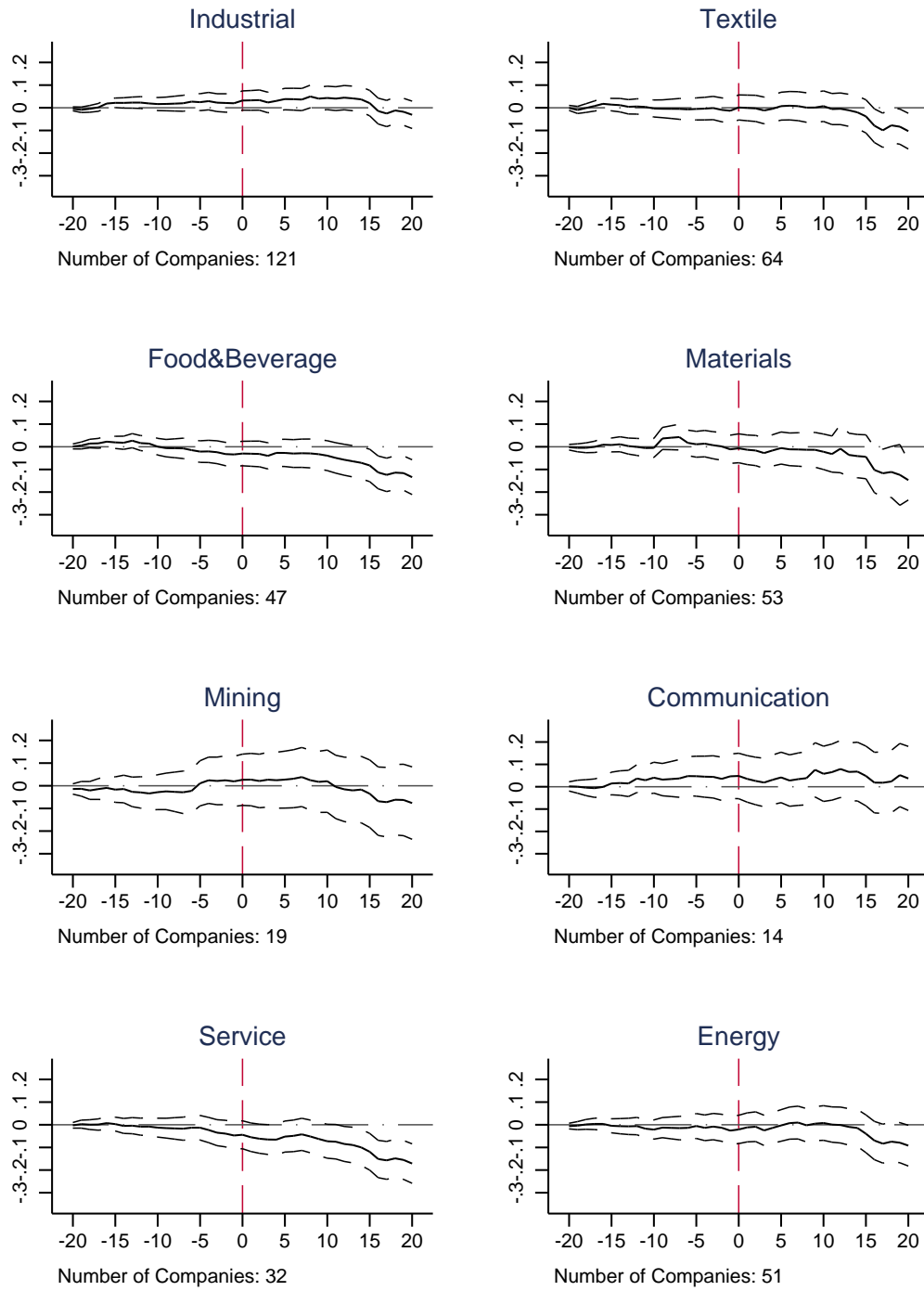
Note: Corresponding to column (1) in Table 1.3, average cumulative abnormal return for all 129 real estate firms,  $\overline{CAR}[-20, k]$  and its 95% confidence intervals are plotted over  $k$ .

Figure 1.5: Average CAR of Real Estate Firms, by Stock Exchange



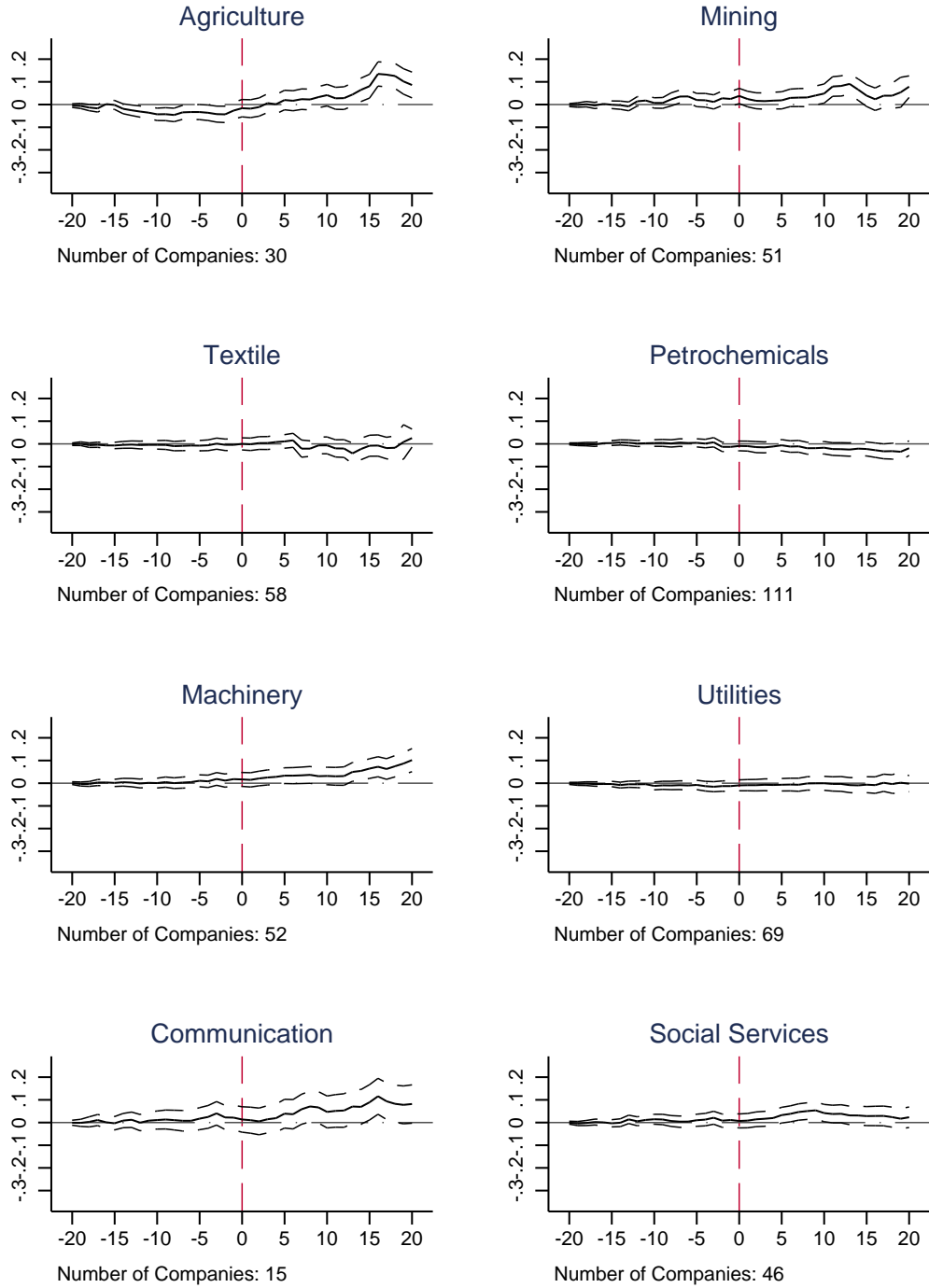
Note: Analogous to Figure 1.4, average cumulative abnormal return is plotted for real estate firms publicly traded on the Hong Kong Stock Exchange (4a) and the two mainland stock exchanges (4b), corresponding to columns (2) and (3) in Table 1.3. The bottom two figures plot the average CAR for the Shanghai and Shenzhen Stock Exchanges, respectively.

Figure 1.6: Average CAR of Non-Real-Estate Firms Listed on the HKEx



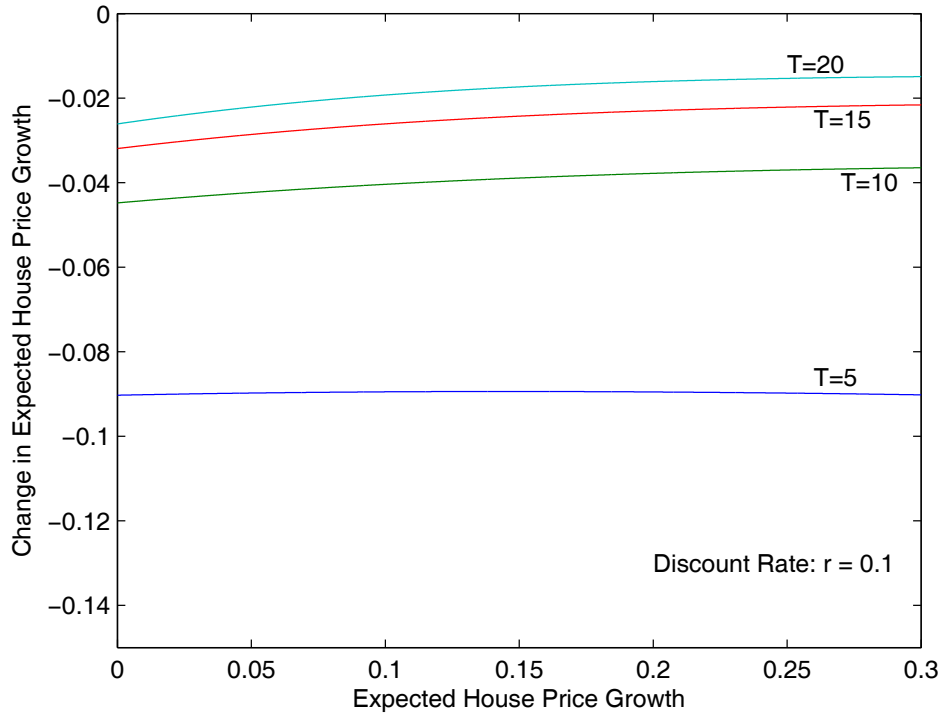
Note: Average cumulative abnormal return,  $\overline{CAR}[-20, k]$  and its 95% confidence intervals are plotted over  $k$  for each of the eight randomly-chosen industries listed on the Hong Kong Stock Exchange.

Figure 1.7: Average CAR of Non-Real-Estate Firms Listed on the SSE and SZSE



Note: Average cumulative abnormal return,  $\overline{CAR}[-20, k]$  and its 95% confidence intervals are plotted over  $k$  for each of the eight randomly-chosen industries listed on the Shanghai and Shenzhen Stock Exchanges.

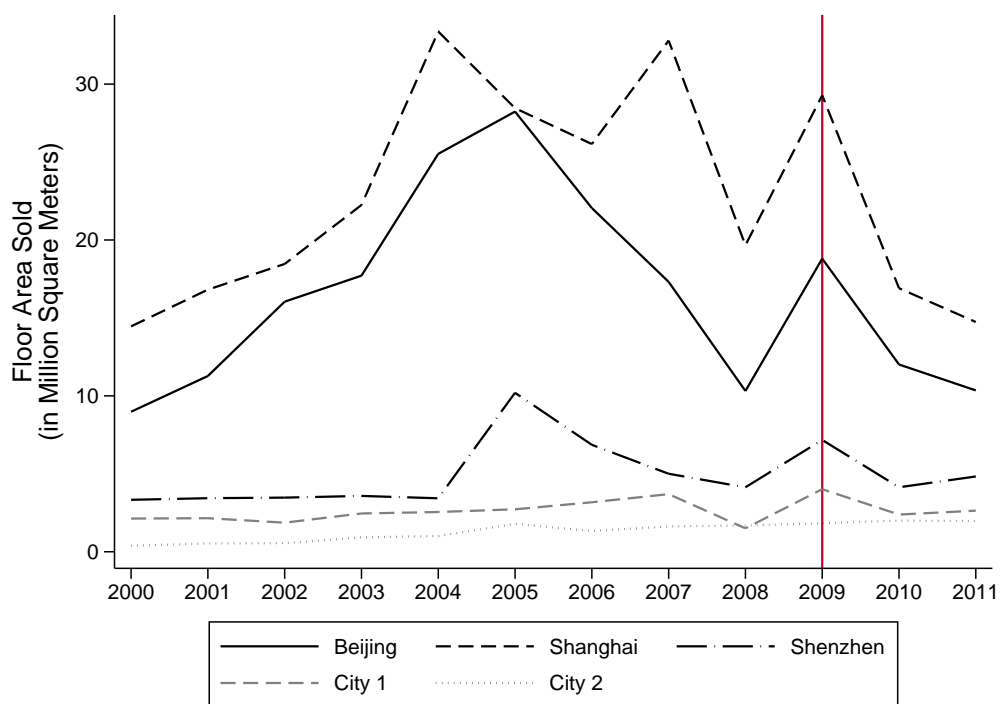
Figure 1.8: Implied Change in Expected House Price Appreciation



Note: This figure plots the simulated change in expected house price appreciation after the announcement of “Document Number 10”. This exercise is based on Equation (2) and assumptions in Section 1.5.2. The horizontal axis represents expected house price growth before the policy announcement. I assume discount rate  $r = 0.1$  and expected years of firm operation  $T = 5, 10, 15, 20$ . Various choices of discount rate (ranging from 0.05 to 0.15) leads to similar results.



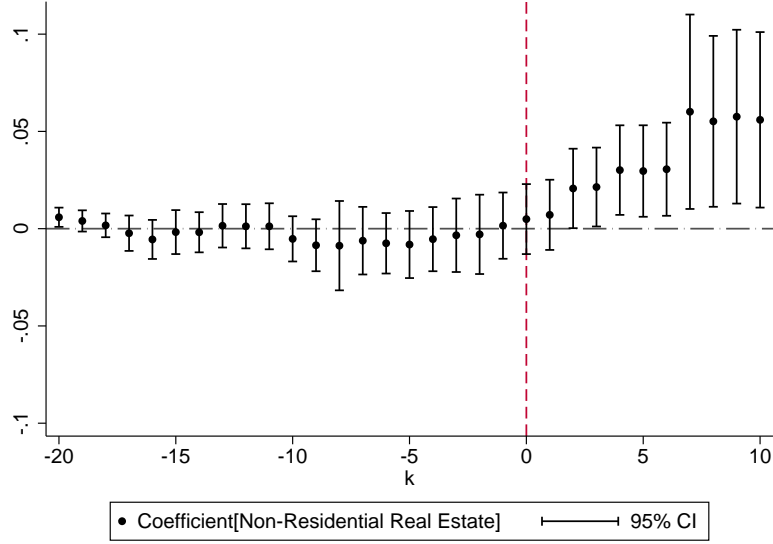
Figure 1.9: Floor Area Sold in Selected Speculative Markets (2000-2011)



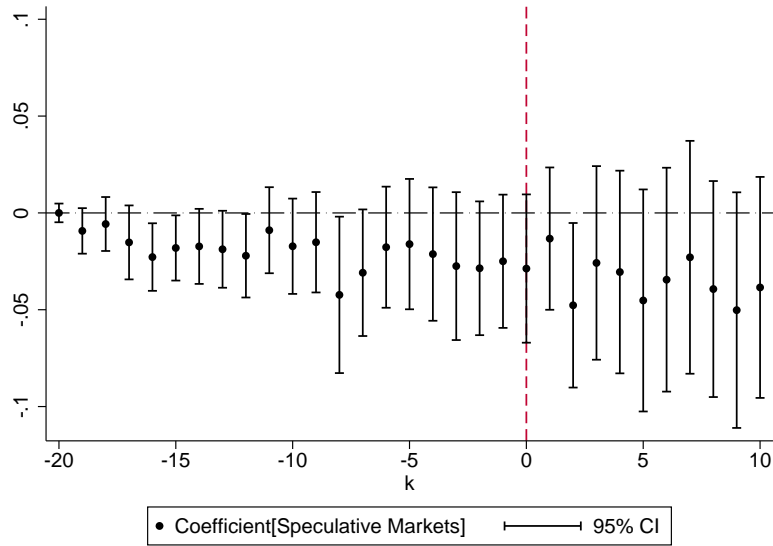
Note: This figure plots the total floor area of residential properties sold in each of the five speculative markets, available from the National Bureau of Statistics. See Section 1.6.1.2 for the definition of “speculative markets” in this paper. Two city names are screened for confidential reasons.

Figure 1.10: Point Estimates over the Course of Policy Announcement

(i) Non-Residential Real Estate Concentration



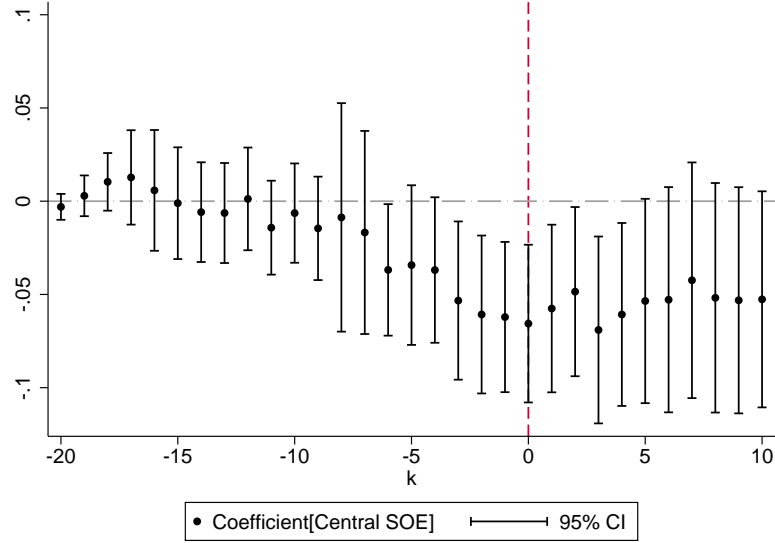
(ii) Speculative Markets



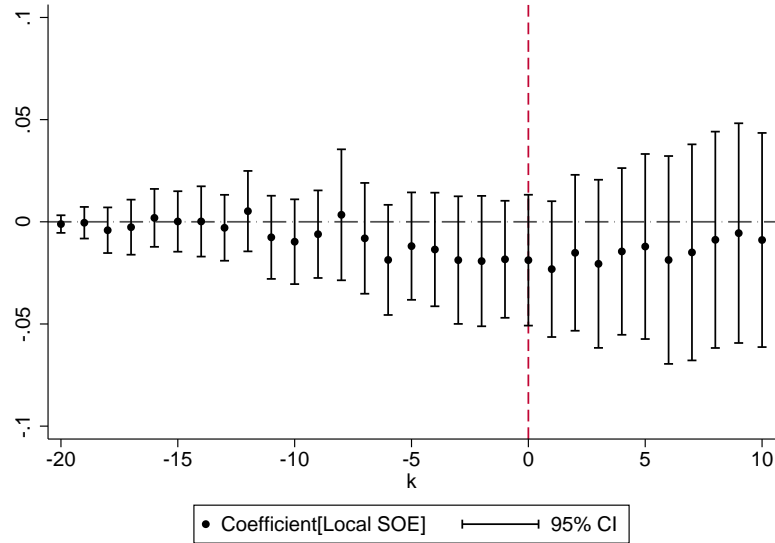
Note: Figures 1.10 plot the coefficients on the two variables for firms' business exposure in non-residential real estate and speculative markets over  $k$  when equation (1.3) is sequentially estimated using  $CAR[-20, k]$  as the dependent variable ( $k \in [-20, 10], k \in Z$ ). The 95% confidence intervals are plotted around the point estimates. All the regressions use the baseline specification from column (3) in Table 1.10.

Figure 1.11: Point Estimates over the Course of Policy Announcement (Cont'd)

(iii) Central State-Controlled Firms



(iv) Local State-Controlled Firms



Note: Figure 1.11 plot the coefficients on the two variables for political connections (Central SOE and Local SOE) over  $k$  when equation (1.3) is sequentially estimated using  $CAR[-20, k]$  as the dependent variable ( $k \in [-20, 10], k \in Z$ ). The 95% confidence intervals are plotted around the point estimates. All the regressions use the baseline specification from column (3) in Table 1.10.

Appendix Table 1.A: Outline of the State Council “Document Number 10”

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I. Local governments and bureaus should fulfill the responsibility of stabilizing local house prices and promoting housing affordability
<ul style="list-style-type: none"> <li>(1) Local governments and bureaus should carry out the central government policies to curb excessive appreciation of house prices</li> <li>(2) A system will be established to assess and evaluate how local governments fulfill these housing-related tasks</li> </ul>

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II. To curb speculative demand for housing
<ul style="list-style-type: none"> <li>(3) For second-home purchases, banks should impose a minimum down payment rate of 50% and a mortgage rate of at least 110% of the base rate. Banks may suspend loans to third-home purchases, and should stop lending to non-resident home purchasers who have not paid taxes in that city for at least 1 year</li> <li>(4) To raise taxes to restrict speculative housing transactions</li> </ul>

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III. To increase effective supply of housing
<ul style="list-style-type: none"> <li>(5) To guarantee the effective supply of land for residential use</li> <li>(6) To guarantee the supply of moderate-sized housing units. At least 70% of the residential land supply should be designated for the construction of affordable and moderate-sized housing units, and the renovation of shanty towns</li> </ul>

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IV. To speed up the construction of government-subsidized housing
<ul style="list-style-type: none"> <li>(7) To construct 3 million government-subsidized housing units and 2.8 million renovated units (from shanty towns) nationwide in 2010</li> </ul>

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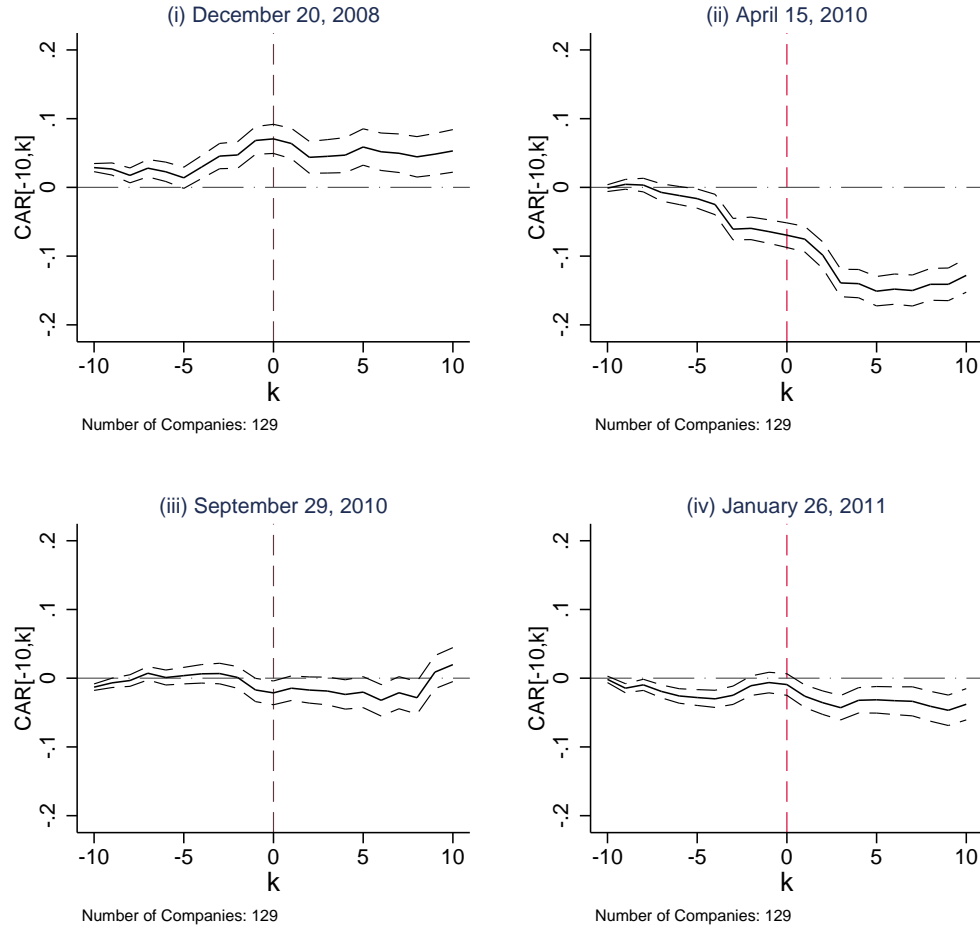
V. To tighten supervision of the housing market
<ul style="list-style-type: none"> <li>(8) To punish violators of housing and land market regulations; banks are urged to tighten lending criteria for real estate firms</li> <li>(9) To punish misbehavior in housing transactions</li> <li>(10) To promote information disclosure and set up housing transaction database</li> </ul>

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<sup>1</sup> Source: the State Council “*Guowuyuan guanyu jianjue ezhi bufen chengshi fangjia guokuai shangzhang de tongzhi*” [Notice of the State Council to Resolutely Curb the Rapid Rise in House Prices in Some Cities], Guofa[2010], No.10.

<sup>2</sup> The official document is available in Chinese at the website of the central government: [http://www.gov.cn/zwggk/2010-04/17/content\\_1584927.htm](http://www.gov.cn/zwggk/2010-04/17/content_1584927.htm).

Appendix Figure 1.A: Average CAR of Real Estate Firms: other Policy Events



Note: Average cumulative abnormal return for all real estate companies,  $\overline{CAR}[-10, k]$  and its 95% confidence intervals are plotted over  $k$ . Four major housing policy announcements from 2008 to 2011 are examined here. Plot (ii) refers to the State Council “Document Number 10”.

## Chapter 2

# The Role of Contagion in the Last American Housing Cycle

(With Anthony DeFusco, Fernando Ferreira and Joseph Gyourko)

## 2.1 Introduction

One of the striking features of the recent U.S. housing cycle is its heterogeneity across markets. Both the magnitudes and timing of price swings varied greatly across metropolitan areas (Sinai (2012)). Figure 2.1 plots the geography and timing of the start of housing booms at the metropolitan area level from 1993 to 2009 based on estimates reported in Ferreira and Gyourko (2011).<sup>1</sup> The top left panel marks the 15 primarily rust belt and interior markets that never boomed. The other panels show that the remaining markets boomed at very different times over a nearly decade-long period from 1997 to 2005. The housing boom spread from what were initially highly concentrated areas on the two coasts, with the earliest booms beginning between 1997-1999 in California and the mid-New England region. On the west coast, housing booms eventually spread inland towards central California and to neighboring states to the east and north. On the east coast, housing booms spread to other markets in New England and then to neighboring regions, eventually reaching the majority of Florida markets by 2004 and 2005. These patterns are suggestive of spillover effects that disseminate positive housing price changes from one market to another.

In this paper we investigate whether contagion was an important element of the last American housing cycle and also directly test which mechanisms are driving the price contagion. In the financial economics literature, a spillover is often referred to as contagion when it is found following a negative shock to one or more countries or markets.<sup>2</sup> While we focus on spillovers from a positive shock in much of this paper, we use the terms spillover and contagion interchangeably in order to emphasize the close

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<sup>1</sup>They define the beginning of a metropolitan area's housing boom by the quarter in which there is a structural break (a discrete positive jump in this case) in that market's house price appreciation rate. This methodology and the rationale behind it are discussed more fully below in Section 2.2.

<sup>2</sup>See Forbes (2012) for an excellent recent review of that literature, and Dungey, et. al. (2005) for a more technical analysis of the challenges involved in convincingly estimating contagion or spillover effects. Previous work on financial market contagion includes studies of the 1987 U.S. stock market crash (King and Wadhwani (1990); Lee and Kim (1993)), the 1994 Mexican peso crisis (Calvart and Reinhart (1996)), and the Hong Kong stock market and Asian currency crisis of 1997 (Corsetti, et. al. (2005)).

intellectual linkage of our work with the analysis of contagion in financial economics. To be precise, we define contagion as the price correlation across space between two different housing markets following a shock to one market that is above and beyond what can be justified by common aggregate trends.<sup>3</sup>

We address several empirical concerns that plague previous contagion-related research. One example involves the determination of the relevant period(s) in which to study contagion or spillovers. A non-*ad hoc* procedure for identifying the timing of a shock would be preferred to an arbitrary choice of a time period ‘after the fact’. This typically is not feasible in most studies of stock market or currency crises because there is little or no variation across countries in the onset of those events. We deal with such problem by appealing to urban economic theory (Glaeser et al.’s (2012) dynamic version of the classic model of spatial equilibrium) to help define when the boom begins in each metropolitan area. As is described more fully in the next section, this leads us to date the beginning of a given market’s boom by whether and when there was a structural break in that area’s price appreciation rate. These estimates, as shown in Figure 2.1, provide substantial variation in the timing of the start of local market booms.

A second advantage is provided by the use of a voluminous micro-level data on U.S. housing transactions.<sup>4</sup> We have over 23 million observations on individual home sales in 99 metropolitan areas dating back to the early 1990s in most cases. This data enables us to address specification search bias of the type identified by Leamer (1978), which arises when the same data is used to identify both the timing of a shock and the magnitude of the volatility during that period. Our strategy uses randomly split samples to separately identify the timing of booms and the magnitude of price

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<sup>3</sup>While there is no single, agreed-upon definition of contagion, this definition is similar in spirit to many used in the financial economics literature. See, for example, Forbes (2012), which emphasizes the distinction between contagion and interdependence, with the latter term reflecting when events in one country affect others in all states of the world, not just after severe negative events.

<sup>4</sup>The property transaction data is collected by DataQuick or by intermediaries from county assessor’s offices and contains a population of all home sales.



volatility in those periods.<sup>5</sup> Most contagion studies in financial economics are not able to deal with this issue because they use a single aggregate stock index in each country. Doing so increases the likelihood of falsely concluding that there are more and bigger booms (or crisis periods) than truly exist.

Third, the richness of our data and the variation in the timing of booms across markets also helps us deal with omitted variable biases in several ways. As is explained more fully later in the paper, we use the time line of a neighbor’s boom as our source of variation in the data to identify contagion effects. Our baseline specification involves regressing a focal market’s price changes on a series of indicators reflecting whether the relevant neighboring market is booming and how proximate in time a given market is to the start of that market’s boom.<sup>6</sup> The added degrees of freedom afforded by the multiple, non-contemporaneous booms we observe also allow us to control for omitted factors that might reflect common economic shocks. We do so through the inclusion of time by census division dummies, lagged price changes, as well as a host of local fundamentals. We also are able to address the Forbes and Rigobon (2002) critique regarding heteroskedasticity, whereby increased volatility in the ‘crisis period’ (when the boom starts, in our context) generates upward bias in correlation coefficients across markets. We adjust for the volatility in prices being higher than normal when the boom starts by directly controlling for the time line of the focal market’s boom. Finally, in an alternative specification, we also address the potential for reverse causality with an instrumental variable approach using further lags of close neighbor’s price changes.

In addition to these improvements in empirical implementation, we directly estimate the importance of several potential mechanisms that could be driving any ob-

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<sup>5</sup>This is the same strategy followed by Card, Mas and Rothstein (2008) in their study of tipping points in residential segregation models.

<sup>6</sup>As expected, this is very important empirically. Naively regressing ‘price on price’ yields contagion estimates that are 3-5 times larger than the results we report below from our preferred specification.

served contagion. Specifically, we investigate the impact of closest neighbor’s housing boom on the average income of potential home buyers, behavior of lenders, migration, and speculative activity in the focal MSA.<sup>7</sup> Moreover, we test whether those mechanisms and other local fundamentals – and expectations of local fundamentals – can explain the magnitude of the contagion effect. Such exercise is relevant since the current literature emphasizes the importance of behavioral factors in the last housing boom.<sup>8</sup>

Our main conclusion is that contagion played a statistically and economically significant role in the development of the most recent housing boom. The elasticity of prices in a typical focal market with respect to those in the closest metropolitan area in the year following the beginning of the boom in that neighboring area ranges from 0.10 to 0.27.<sup>9</sup> Empirically, the upper end of our elasticity range implies that from one-fourth to one-third of the average jump in price growth at the start of a typical local boom was due to contagion effects. This average impact is driven entirely by the physically closest neighbor – and is only detected if the nearest neighbor had a statistically significant housing boom. There is no evidence of spillovers on prices arising from more geographically distant markets. In addition, this impact does not vary materially with the number of miles between the focal market and its nearest neighbor.<sup>10</sup> As an additional robustness, we also find evidence of contagion on the

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<sup>7</sup>The finance literature has also investigated the channels of contagion in the context of currency or stock market crises via international trade, financial institutions and portfolio investors, for example. See Forbes (2012) for a good review on that thread of literature.

<sup>8</sup>See, for example, Case and Shiller (2003) and Case, Shiller and Thompson (2012).

<sup>9</sup>We also report evidence of on-going contagion effects as the boom builds, but those results are potentially confounded by feedback effects. This is not a concern at the start of local booms because the data show no pre-trends, with markets appearing to be on their equilibrium paths before the initial jump in price growth when the booms commenced.

<sup>10</sup>We also employed an economic measure of distance based on migration flows between metropolitan areas that has been used in other research (Sinai and Souleles (2009)). It is strongly positively correlated with geographic distance between markets. For example, the probability of the physically closest neighbor also being the closest economic neighbor is 57%, and the probability it is one of the two closest economic neighbors is 76%. Hence, it is not surprising these two measures of distance yield similar results. Consequently, we do not report those regression findings for space reasons, but do discuss the rare exceptions when different findings result from use of the two different distance metrics.

extensive margin. Hazard models show that the probability of a boom beginning this quarter is indeed influenced by close metros that boomed in the previous quarter.

We also investigated heterogeneity in the price contagion along a number of other non-distance related dimensions. For example, one might expect contagion to be stronger when the transmission is from larger neighbor to a smaller focal market. That is precisely what we find. Contagion also varies with the elasticity of supply of the metropolitan area. Specifically, there is no evidence of contagion in inelastically supplied markets, but the estimated impact in the most elastically supplied markets is double the average effect discussed above.<sup>11</sup> As an additional robustness test, we also find contagion in the extensive margin – hazard models show that the probability of a boom beginning this quarter is indeed influenced by close metros that boomed last quarter.

What mechanisms explain the price contagion effect? Fundamental factors of the focal MSA, including local incomes, migration flows, lending behavior, and speculative behavior, do not show much variation after the beginning of the boom of the nearest neighbor. We also include a simple form of expectations of these local market fundamentals in our main econometric model to see if this materially affects the magnitude of the estimated contagion effect. It does not. This indicates that at least some of the contagion we estimate may be due to forces not related to the fundamentals analyzed in this paper. This has potentially important implications for policy makers. To the extent that the spread of a housing boom is even partially due to non-fundamental forces (e.g., some type of irrational exuberance or otherwise mistaken perceptions of the influence of a neighboring market), it may be worth rethinking the advisability of not responding to a boom.

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<sup>11</sup>This particular heterogeneity certainly should not be given any causal interpretation, as it turns out that a large fraction of markets in inelastic supply are large relatively to their neighbors, for example. Nevertheless, the result of contagion playing an immaterial role in inelastically supplied markets is consistent with prior findings in Ferreira and Gyourko (2011), which showed that the household income of mortgage applicants jumps enough to account for virtually all of the initial booms in those markets.

Despite finding important spillovers during the U.S. housing boom, we report mixed evidence that contagion played a role in the bust. Estimated elasticities are zero at the beginning of the bust, but they increase to about 0.15 by the third year of the housing bust of the nearest neighbor. We also cannot detect any impact of contagion on the extensive margin during the bust. This is perhaps not all that surprising since the timing of the bust across MSAs is heavily concentrated in an 18 month period during 2006 and 2007, while the buildup of the housing boom took almost a decade. This highlights difficulties in detecting spillovers during economic or financial crashes/booms that quickly spread across countries, regions, or firms.<sup>12</sup>

While our research is motivated by prior research in financial economics,<sup>13</sup> it is also part of the growing body of work on the most recent housing cycle. One important strand of that research tries to understand whether the most recent cycle was a bubble.<sup>14</sup> Another more voluminous body of papers analyzes the bust and its consequences.<sup>15</sup> There are also some prior studies of price spillovers in the housing

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<sup>12</sup>In addition, we do not have the added advantage of relying on a prediction from external theory to date the beginning of the bust. In this part of the analysis, we follow the tradition in the contagion literature and date the bust's beginning in an *ad hoc* manner based on the assumption that it begins in the quarter after nominal price levels peaked in a given metropolitan area.

<sup>13</sup>Forbes and Rigobon (2002) define contagion as a "significant increase in cross-market linkages after a shock to one [market]." Others have used this definition (or something similar) to study financial contagion in many contexts including the 1987 U.S. stock market crash (King and Wadhwani, 1990; Lee and Kim, 1993), the 1994 Mexican peso crises (Calvo and Reinhart, 1996), and the Hong Kong stock market crisis of 1997 (Corsetti et al., 2005).

<sup>14</sup>Shiller (Chapter 2, 2005) provides perhaps the most famous characterization of the boom as a non-rational event. Others recently have estimated rational expectations general equilibrium models to try to explain the national aggregate price data (e.g., Favilukis, Ludvigson, and Van Nieuwerburgh (2011)) or the serial correlation and volatility of prices and quantities within and across metropolitan areas (e.g., Glaeser et al. (2012)). Related work includes Arce and Lopez-Salido (2011), Burnside, Eichenbaum and Rebelo (2011), Lai and Van Order (2010), and Wheaton and Nechayev (2008).

<sup>15</sup>Much of this research focuses on the subprime sector (e.g., Bajari, Chu and Park (2008), Danis and Pennington-Cross (2008), Demyanyk and Van Hemert (2011), Gerardi, Shapiro and Willen (2007), Goetzmann, Peng and Yen (2009). Mayer and Pence (2008), Haughwout, et. al. (2011)), mortgage securitization (e.g., Bubb and Kaufman (2009), Keys et. al. (2010)), the default/foreclosure crisis (e.g., Adelino, Gerardi and Willen (2009), Campbell, Giglio and Pathak (forthcoming), Foote, Gerardi and Willen (2008), Gerardi et. al. (2008), Mayer, Pence and Sherlund (2009), Mian and Sufi (2009), Mian, Sufi and Trebbi (2010), Piskorski, Seru and Vig (2009)) or the role of government regulation (e.g., Avery and Brevoort (2010), Bhutta (2009), Ho and Pennington-Cross (2008)).

market.<sup>16</sup> In this paper, we focus on one particular facet of the cycle, the role of contagion, but contributed to the literature by: (a) looking at contagion over the full time span of the cycle; (b) improving empirical identification in many respects; (c) estimating heterogeneity in the contagion elasticity; and (d) investigating the potential causes of contagion across housing markets.

The plan of the paper is as follows. The next section motivates our use of an urban economic approach to analyzing contagion in housing markets and discusses our method for dating the beginning of the boom. Section 2.3 describes the various data sources employed and the variables created. Section 2.4 discusses the different types of specifications estimated, reports results, and explores potential mechanisms that might explain the contagion effects. In Section 2.5 we look at alternative specifications, the extensive margin of contagion and whether we can find contagion at the housing bust. There is a brief conclusion.

## 2.2 An Urban Economic Motivation and Definition of Contagion

### 2.2.1 Timing of Local Housing Booms

Any analysis of possible contagion effects in the spreading of the recent housing boom first requires knowledge of the timing of the beginning of the boom in different

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<sup>16</sup>Some early examples include Clapp, et al. (1995) and Dolde and Tirtiroglu (1997), who use data on towns in Connecticut and San Francisco (respectively) to test for the existence of cross-market linkages in price movements. More recently, Holly, et al. (2010, 2011) use data on U.S. states and U.K. regions to study the spatial and temporal diffusion of changes in house prices during the most recent cycle. Fuss, et. al. (2011) and Cotter, et. al. (2011) use publicly available, metropolitan area-level house price series to test for the existence of some form of contagion during the most recent housing cycle. However, all studies use the same aggregate market-level price data to determine both the timing of the crisis period and to measure the magnitude of volatility changes during that period, which makes their estimates susceptible to specification search bias. In addition, the timing of the shock is usually defined in an *ad-hoc* way and there are questions about how that research deals with omitted factors.

markets. Our data on the time line of local booms come from estimates reported in Ferreira and Gyourko (2011). In that work, the start of local booms is determined by when there was a structural break in each area’s price appreciation rate series.

The justification for that strategy is based on implications of the dynamic spatial equilibrium model developed in Glaeser, et al. (2012).<sup>17</sup> In particular, that model implies that in steady state each local market will exhibit constant and continuous growth paths for house prices, new construction and population.<sup>18</sup> Empirically, this means that we should see house prices in a given market growing at a constant rate unless there is a shock to local productivity or amenities, in which case we would then observe a discrete jump in the appreciation rate for that market. The data are consistent with this predicted pattern, as illustrated in Figure 2.2’s depiction of house price appreciation rates over time for the Las Vegas market. This graph, which is taken from Ferreira and Gyourko (2011), shows that fast-growing market to be appreciating at a high, but roughly constant rate, for many years before house price growth escalates sharply at the beginning of its boom. Informally, we take the point at which house price growth rates exhibit this discrete jump to be the beginning of the housing boom.

The temporal distribution of the quarter in which booms began in all 99 MSAs in our sample was plotted in Figure 2.1. While we refer interested readers to Ferreira and Gyourko (2011) for the details of the formal procedure underpinning these estimates, a very brief summary is as follows. We start with the following reduced form model

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<sup>17</sup>Glaeser, et al. (2012) introduce dynamics into Rosen’s (1979) and Roback’s (1982) classic static model of spatial equilibrium. In this compensating differential framework, house prices ( $P_i$ ) are the entry fee paid to access the wages ( $W_i$ , which reflect productivity) and amenities ( $A_i$ ) of labor market area  $i$ . Their model is closed with an assumption that there is some elastically supplied reference market area which is always open to another household. The utility level available in the reference market is given by  $U^*$ , and establishes the lower bound on utility provided in any market. In the long run, perfect mobility ensures that  $U^*$  is achieved in all markets, so that in equilibrium, no one has an incentive to move to another place which offers higher utility. A very simple, linear version of this framework would imply that  $U^* = W_i + A_i - P_i$ , so that  $dP_i = dW_i + dA_i$  in equilibrium.

<sup>18</sup>The steady state rate of price appreciation need not be zero. Secular trends in house prices can come from an underlying trend in housing demand as long as the market is not in perfectly elastic supply. It can also arise from trends in physical construction costs under certain conditions.

of steady state house price growth in MSA  $i$  at time  $t$ :

$$PG_{i,t} = d_{i,t} + \varepsilon_{i,t}, \quad t = 1, \dots, T. \quad (2.1)$$

Glaeser, et al. (2012) implies that  $d_{i,t} = d_0$  for all  $t$  if the market is on its steady-state growth path and is not being shocked in any way. Thus, the beginning of a local housing boom can be identified by testing for the existence of one or more structural breaks in the parameter  $d_{i,t}$ . To do this we follow well-established methods in the time series literature for estimating structural breaks.

Borrowing heavily from Estrella's (2003) notation, the null hypothesis is that  $d_t$  is constant for the entire sample period:

$$H_0 : \quad d_{i,t} = d_0, \quad t = 1, \dots, T.$$

The alternative is that  $d_{i,t}$  changes at some proportion,  $0 < \pi < 1$ , of the sample which marks the beginning of a housing boom. Specifically,

$$H_1 : \quad d_{i,t} = \begin{cases} d_1(\pi), & t = 1, \dots, \pi T \\ d_2(\pi), & t = \pi T + 1, \dots, T. \end{cases}$$

For any given  $\pi$ , it is straightforward to carry out this hypothesis test. However, things are slightly more complicated when  $\pi$  is unknown and the determination of its value is the primary interest, as is true in our case.

To see how we estimate the value of  $\pi$  and assess its statistical significance, let  $\Pi = [\pi_1, \pi_2]$  be a closed interval in  $(0, 1)$  and let  $S$  be the set of all observations from  $t = \text{int}(\pi_1 T)$  to  $t = \text{int}(\pi_2 T)$ , where  $\text{int}(\cdot)$  denotes rounding to the nearest integer. The estimated break point is the value  $t^*$  from the set  $S$  that maximized the likelihood ratio statistic from a test of  $H_1$  against  $H_0$ .<sup>19</sup>

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<sup>19</sup>We use the terms supremum and maximum interchangeably in this exposition. Technically, all of the results are in terms of the supremum of the likelihood ratio statistic.

Assessing the statistical significance of this estimate requires knowing the distribution of the supremum of the likelihood ratio statistic as calculated from among the values in  $S$ . Let  $\xi = \sup_S LR$  denote this supremum. Andrews (1993) shows that the distribution of  $\xi$  can be written as

$$P(\xi > c) = P\left(\sup_{\pi \in \Pi} Q_1(\pi) > c\right) = P\left(\sup_{1 < s < \lambda} \frac{\|B_1(s)\|}{s^{1/2}} > c^{1/2}\right) \quad (2.2)$$

where  $\|B_1(s)\|$  is the Bessel process of order 1,  $\lambda = \pi_2(1 - \pi_1)/\pi_1(1 - \pi_2)$ , and

$$Q_1(\pi) = \frac{(B_1(\pi) - \pi B_1(1))'(B_1(\pi) - \pi B_1(1))}{\pi(1 - \pi)}.$$

Calculation of the probability in Equation (2.2) is non-trivial. However, Estrella (2003) provides a numerical procedure for doing so. Using that method, we are able to calculate p-values for the estimated break points for each MSA in the sample.

Note that this procedure generates a breakpoint estimate regardless of whether the structural break represents a positive or negative change in the price growth rate. In the cases where the estimated break point is either insignificant or implies a negative change in growth rates, we conclude that the market did not have a boom. That is the case for the 15 interior markets shown in the first panel of Figure 2.1. For those locations where we do find evidence of a statistically significant and positive break point, we then proceed to test for the existence of two breaks against the null of only one. To do so, we closely follow Bai (1999) and Bai and Perron (1998) and we refer the reader to Appendix 2.7.1 for the details of this procedure. A few MSAs were found to have experienced more than one structural break. However, for almost all of those cases, the secondary breaks were small economically or not significantly different from zero. Moreover, the estimation of a secondary break generally does not displace the location of the main structural break. Therefore, we only use the



estimation of one main structural break in this paper.

### **2.2.2 Contagion in Local Markets**

A potential role for neighbors to influence house price growth in a focal market arises naturally within the dynamic urban model of spatial equilibrium referenced above. While users of that framework typically presume that shocks originate from own market fundamentals, they could arise from neighboring markets as well. In our case, we are interested in whether neighbors that just had housing booms may impact housing outcomes in the focal metropolitan area, all else constant. Such contagion effects could arise due to fundamental, informational, or psychological factors.

An example of a fundamental factor generating spatial spillovers is a positive industry or income shock that triggers a housing boom in one local labor market. For example, if there is such a shock in the Silicon Valley, house prices in the San Jose-Sunnyvale-Santa Clara MSA will increase in the short run given supply constraints, and perhaps start a housing boom. Neighboring areas, such as the San Francisco-Oakland-Fremont MSA (or smaller, but more distant, metropolitan areas in the central valley of California), may eventually benefit from that positive income shock, as some of the Silicon Valley jobs could migrate to nearby areas. Even though such fundamental spillovers may occur with lags, house prices in the neighboring markets should immediately capitalize the expectation of future economic growth.

Housing market shocks could also be disseminated through the credit market channel. In the example above, Silicon Valley lenders may achieve extra profits since foreclosures and delinquencies tend to decline during an economic boom. If those lenders decide to reinvest profits and expand market shares in a nearby MSA possibly because it is less costly to expand business to nearby communities then the San Francisco metro may observe a shift in the availability of credit, which will boost

its housing market both in the short and in the long runs.<sup>20</sup> A similar mechanism could exist for land owners and housing investors in the Silicon Valley. Their wealth increases after the beginning of the Silicon Valley housing boom, which could trigger an expansion of investments into neighboring MSAs.<sup>21</sup>

Spillovers could arise even in the absence of large realized future fundamental changes in San Francisco or the central valley. Residents in those neighboring markets may be right to think that some type of positive income spillover will occur from the Silicon Valley boom, but they may incorrectly predict its magnitude. Those biased expectations can lead to short-run increases in their housing prices.<sup>22</sup> Additionally, irrational factors may lead residents in the focal market to have not only incorrect, but also non-fundamentally based expectations about future price growth in their market following a shock to a nearby area.

Finally, the housing boom in the Silicon Valley may generate another type of psychological spillover effect. An increase in Silicon Valley housing prices may lead their own local residents to pay more attention to what is happening in neighboring markets, such as the city of San Francisco. Therefore, a shock that makes the focal housing market and its interactions with neighbors more salient to investors may itself lead to stronger contagion. Before discussing the econometric specifications to identify contagion and its mechanisms, we next detail our main datasets.

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<sup>20</sup>Ortalo-Magne and Rady (2006) show how increases in the availability of credit to marginal buyers can lead to overreaction in house prices.

<sup>21</sup>Chinco and Mayer (2012) report that out-of-town speculators played a significant role in the housing boom and that their presence may have exacerbated price increases in some markets. Bayer et al. (2011), similarly document the rise of speculative activity during the housing boom.

<sup>22</sup>A similar mechanism underlies the analysis in King and Wadhvani (1990) who document that contagion in financial markets can arise as a result of attempts by rational agents to infer information from price changes in other markets. Similarly, Clapp, et al. (1995) document characteristics of house price dynamics that could be consistent with rational learning. Burnside et al. (2011) and Favara and Song (2011) show how the presence of optimistic agents in the housing market can lead to increases in house price levels and volatility.

## 2.3 Data

Our house price data come from DataQuick, a private data vendor which collects the universe of home transactions across the country. The sample used is for 99 metropolitan areas, with information on over 23 million individual observations ranging from the first quarter of 1993 through the third quarter of 2009. We randomly split the sample into two and in each subsample, we create a constant quality quarterly price index for each MSA.<sup>23</sup> One of these indices is then used to estimate timing of the boom according to the structural break analysis described above, and the other is used to analyze the magnitude of price changes following housing booms in neighboring markets. We adopt this strategy in order to avoid the specification search bias identified by Leamer (1978).<sup>24</sup> The mean, standard deviation, and interquartile range for the price index we use to measure magnitudes are reported in the first row of Table 2.1.

We also create a number of variables to measure fundamentals that are potentially correlated with house price growth and the timing of the beginning of local housing booms. We consider three types of fundamentals: (1) demand shifters, such as the average income of mortgage applicants, MSA-level unemployment rates, and net migration flows; (2) buyer characteristics and property traits, including the percentage of speculators, the percentage of minority buyers and the average square footage of transacted housing units; and (3) credit market conditions, measured by the average loan-to-value ratio of home purchases, the percentage of mortgages originated by

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<sup>23</sup>We create a MSA-level constant quality house price series by quarter using hedonic regressions. Price, in its logarithmic form, is modeled as a function of the square footage of the home entered in quadratic form, the number of bedrooms, the number of bathrooms, and the age of the home. We also created a version of the Case and Shiller (1987) repeat sales price index for 14 Case-Shiller markets that overlap with the DataQuick files, and found that the simple correlation of appreciation rates on the two different indexes based on DataQuick is usually higher than 0.9. We employ hedonic price indexes because their data requirements are much less onerous.

<sup>24</sup>The bias from not doing this is large. For example, if we use the full DataQuick sample to estimate both the timing of the beginning of the boom and the magnitude of the jump in price growth at that time, the estimated jumps double those arising from a split-sample estimation.

subprime lenders and those insured by the Federal Housing Administration (FHA).

To construct many of the demographic measures of home buyers, we merge the DataQuick files with the Home Mortgage Disclosure Act (HMDA) data, which provide information on the income and race of all mortgage applicants (not just those who actually bought homes). In each time period, we calculate the average income of all local loan applicants as reported in HMDA. Similarly, the “Percent Minority” variable reflects the fraction of African-American and Hispanic loan applicants as coded in the HMDA files.

MSA-level unemployment rates come from the Bureau of Labor Statistics’ *Local Area Unemployment Statistics* series, and net migration flows are calculated using data on county-to-county migration patterns provided on an annual basis by the Internal Revenue Service.

“Percent Speculators” refers to the fraction of transactions involving a speculator on either the buyer or the seller side of the transaction. Similar to Bayer, et al. (2011), we define a person as a speculator if he or she is observed to have ‘flipped’ at least two homes in the same metropolitan area during the entire course of the sample, where a flip is defined as a purchase and sale of the same home within a two-year window.

Credit market variables include the average loan-to-value ratio (LTV) among homebuyers in DataQuick, the fraction of FHA-insured loans, and the fraction of subprime loans. We use information on the underlying mortgage lenders from the DataQuick files to calculate the share of subprime loans. More specifically, we obtained lists of the top twenty subprime lenders from 1990-onward in a publication now called *Inside Mortgage Finance*.<sup>25</sup> “Percent subprime lenders” is then defined as the share of mortgages issued by these top twenty lenders.

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<sup>25</sup>This publication claims to capture up to 85% of all subprime originations in most years. Previously, it was named *B&C Mortgage Finance*. See Chomsisengphet and Pennington-Cross (2006) for more details on these lenders and lists. Other papers such as Mian and Sufi (2009) and Keys, et al. (2010) have access to micro-level FICO scores and use that to define subprime borrowers.

As a control for changes in the composition of houses transacted, we also calculate the average square footage of homes sold. This variable hardly changes during the sample period. Note also that changes in the composition of transacted homes should not materially affect our estimates given that we control for an extensive set of housing characteristics when constructing the hedonic price series.

## 2.4 Econometric Model and Estimates

### 2.4.1 Econometric Model

As discussed in Section 2.2, the implications of the underlying dynamic model of urban economics are readily extended to include shocks from neighboring housing markets. We estimate the following reduced form model to gauge the impact of housing market booms of close neighbors on the prices of the focal MSA  $m$  in Census Division  $d$  and quarter  $t$ :

$$\begin{aligned} \log(P_{m,d,t}) = & \sum_{r=-4, r \neq 0}^4 \theta_r^1 \cdot \psi_{m,r,t}^1 + \sum_{r=-4, r \neq 0}^4 \theta_r^2 \cdot \psi_{m,r,t}^2 + \sum_{r=-4, r \neq 0}^4 \theta_r \cdot \psi_{m,r,t} \\ & + \sum_{\tau=1}^4 \theta_\tau \cdot \log(P_{m,d,t-\tau}) + \gamma_{d,t} + \varepsilon_{m,d,t} \end{aligned} \quad (2.3)$$

The first term on the right-hand side of equation (2.3) contains the primary variables of interest, the  $\psi_{m,r,t}^1$ 's, which are indicators for the years relative to the beginning of the housing boom of the closest neighboring market (neighbor number 1). The coefficients,  $\theta_r^1$ , on these indicator variables describe how prices in the focal market evolve over the course of its nearest neighbor's housing boom. We define Relative Year [0] to be the 12-month period prior to the beginning of the neighboring market's boom.<sup>26</sup> Relative year [1] then includes the quarter in which the boom starts

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<sup>26</sup>We work with 12 month periods because there is noise in the quarterly data that is not due

as well as the subsequent three quarters. Relative years from -2 to +3 are entered individually, while all relative years greater (or less) than those numbers are binned together.<sup>27</sup> This allows us to see whether there are any important pre-trends and to track the build-up of the boom after it starts. The second term on the right-hand side of equation (2.3) includes an analogous set of controls, denoted  $\psi_{m,r,t}^2$ , for the second closest neighbor. In general, the effects for the second neighbor turn out to be both economically and statistically insignificant in most specifications, so we report only the coefficients on the nearest neighbor. In a set of robustness tests reported below, we also control for log prices of other near MSAs, to make sure that our elasticity estimates from the nearest neighbor are not confounded by other neighbors, or other neighboring shocks.

Physical proximity is the most natural measure of distance, and the specifications reported below assign the nearest neighbor based on the number of miles between the centroids of the relevant metropolitan areas.<sup>28</sup> As noted in the introduction, we also experimented with a measure of proximity based on migration flows between pairs of markets. Those results were qualitatively and quantitatively similar, so we do not report them for space reasons.

This specification also controls for the time line of the focal market’s boom via the third term on the right-hand side of equation (2.3). By including the relative year effects of the focal MSA (denoted  $\psi_{m,r,t}$ , where the absence of a superscript indicates

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solely to error in the estimation of the break point. For example, it is common for there to be at least a one quarter difference between the time that a transactions price is agreed upon and when the actual closing occurs. In addition, we know that prices in housing markets do not follow a random walk, but move slowly and are strongly positively correlated over short horizons (Case and Shiller (1987, 1989)). Locations for which we do not estimate a statistically significant boom are still assigned a relative year according to their estimated break points.

<sup>27</sup>We did estimate all our models with lengthier spans of individual relative years controlled for, but they did not yield any new insights beyond those reported below.

<sup>28</sup>Distances are calculated using the full set of MSAs according to the 2000 Census. Because we only have price data for 99 of these MSAs, some data for nearest neighbors remain empty in 24 cases. In our regressions, we create an indicator for whether we have price data for the nearest neighbor, and interact this indicator with the relevant relative years. Fortunately, the results are qualitatively similar when we drop MSAs with missing neighbors’ price data and also when we calculate distances using only the 99 MSAs for which we have data.

the variable refers to the focal market itself), we control for the average increase in prices of the focal market over the course of its own boom. This vector serves a similar role to the adjustments made in related financial economics research to deal with upward bias in contagion estimates arising from volatility being higher in all markets during ‘crisis’ periods (e.g., Forbes and Rigobon (2002)).<sup>29</sup>

The remaining terms of equation (2.3) include four quarterly lags of logarithm of focal market prices to control for potentially unobserved time-varying characteristics of MSA  $m$ ,<sup>30</sup> as well as Census Division-by-quarter fixed effects to deal with common regional shocks that might influence close neighbors simultaneously. Finally, note that we do not control for contemporaneous focal market fundamentals in this baseline specification. This is because they could represent intermediate outcomes through which the contagion effect may be operating. In the mechanisms section below, we will directly estimate the impact of the neighbor’s housing boom on those intermediate outcomes, and also test whether their inclusion in equation (2.3) mitigates the contagion effect.

## 2.4.2 Main Estimates

Column 1 of Table 2.2 reports baseline estimates of equation (2.3) for the metropolitan areas whose nearest neighbors had statistically significant booms. The reported coefficients show that prices were relatively stable in the three years prior to the beginning of the boom in the nearest neighboring MSA, so that there is no evidence

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<sup>29</sup>These controls do have their expected impact. If we do not control for the time line of the focal market’s boom, the point estimates of the nearest neighbor’s contagion effects are about 20% higher than those reported just below for our baseline specification in Table 2.2. However, the results are not statistically different once the standard errors are taken into account. While this is consistent with price volatility being artificially high when the focal market itself is booming, we do note that including these indicators could also be controlling for intermediate outcomes. In any event, including this vector is not biasing us towards finding contagion where none exists.

<sup>30</sup>We also considered specifications that dispense with the lags of the dependent variable in favor of a MSA fixed effect. Results from these specifications are qualitatively similar. However, we believe that the lagged dependent variable specification is more appropriate given that omitted time-varying common factors are more likely to confound the contagion effect than unobservable but fixed MSA-specific characteristics.

of a pre-trend. In the year that the neighboring MSA begins its boom (Relative Year [1]), focal market prices then jump 0.87 percentage points and remain almost 1% higher for another couple of years. Column 2 reports the analogous coefficients on the relative year dummies for the set of neighboring MSAs for which we do not estimate a statistically significant or positive break point. These results confirm that a positive effect is only detected when the neighboring MSA actually had a housing boom. Hence, we find spillovers on focal market prices only if the nearest neighbor actually experienced a significantly positive shock. While not reported here for space reasons, the coefficients on the time line of the boom for the 2<sup>nd</sup> nearest neighbor generally are not statistically different from zero regardless of whether this particular neighbor had a housing boom. Hence, spillovers arise only from the closest neighbor.

In order to determine the elasticity of focal market housing price growth with respect to near neighbors' price growth, we need to gauge the magnitude of the housing boom for the nearest neighbors. The starting point of this exercise is to estimate a version of equation (2.3) that uses the log price of the nearest neighbor as the dependent variable. Table 2.3 reports those results. Pre-boom prices are trending down a bit in these results, and then jump 3% in the first year of the housing boom. By the third year of the boom, prices are 8% higher than the pre-boom period, and are more than 11% higher in subsequent years.<sup>31</sup>

An upper bound on the implied elasticity can be computed by using only the estimates of price changes in the first year of the boom under the assumption that agents in these markets are myopic. Combining these figures with the estimates from Table 2.2 yields an elasticity of 0.27.<sup>32</sup>

While differences in data and methodologies make it difficult to directly compare

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<sup>31</sup>These magnitudes are similar to those in Ferreira and Gyourko (2011) who conduct a similar exercise using the full set of MSAs, not just those who serve as closest neighbors to some other MSAs in the sample.

<sup>32</sup>This is the result of dividing the 0.008688 spillover estimate from column 1 of Table 2.2 by 0.03224 from column 1 of Table 2.3.



our estimates of contagion to others in the literature, our magnitudes appear to be smaller and we find that spillovers only arise from the closest neighbors.<sup>33</sup> We suspect this is partially due to our empirical strategy that minimizes specification search bias and includes various controls to deal with omitted factors related to common shocks. Some of the differences also could be due to the current focus of our work—namely, the dissemination of the boom rather than the spread of the bust.

A rough gauge of the relative importance of contagion in fomenting local booms can be made as follows. Ferreira and Gyourko (2011) concluded that jumps in one fundamental, local income growth, could account for one-half of the magnitude of the jump in price growth at the beginning of local booms (on average). Our estimates indicate that contagion did not play as important a role, but it still was economically meaningful, as it can account for over one-quarter of the jump at the start of the boom.<sup>34</sup> Using different and more conservative figures and assumptions reduces the share, but nothing reasonable can drive it below 10%.<sup>35</sup>

Finally, the economic interpretation of the contagion estimates for the years after the beginning of the boom may be more complicated due to potential feedback effects. Feedback effects are less of a concern in the first year of the boom, as we showed that prices, roughly speaking, are in equilibrium right before that moment. It may not

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<sup>33</sup>For example, Cotter, et al. (2011) regress housing price appreciation in eleven MSAs near San Francisco on the contemporaneous and 3 quarterly lags of San Francisco’s housing appreciation rate and report coefficients ranging from 0.05-0.67 on lagged housing price appreciation of San Francisco. Fuss, et al. (2011) model the volatility spillover intensity and suggest that a 1 percentage point shock in housing return in Las Vegas could result in an eventual 0.19 percentage point increase in housing return in San Diego.

<sup>34</sup>To see this more clearly, start with the 0.27 elasticity just discussed. Given the 3.2% average jump in Relative Year [1] reported in Table 2.3, that aforementioned elasticity implies that about 0.9 points, or about 27 percent of the jump at the start of the boom, can be explained by contagion.

<sup>35</sup>It is not clear what figure to use for the average level of price growth in the denominator of this ratio, which is why we focus more on the elasticity. The 3.2% number used here based on Table 3’s results is close to the log price changes reported in Ferreira and Gyourko (2011). However, one also could use the 6.5% average jump in price growth rate (not the change in the log prices) also reported by those authors. That number arises from an estimate that does not control for any year or metropolitan area fixed effects. With that denominator, the 0.9 points amounts to about 14% of the jump at the start of the boom. As noted in the text, there are no reasonable assumptions one could make that drive the share below 10%.

play a major role in subsequent years either, especially if only 10% of the main price effect propagates across close neighbors. Nonetheless, contagion estimates for relative years two and three, for example, are better thought of as reduced form estimates that include the impact of recent contagion, but that also embed a share of contagion from the complete path of price appreciation since the beginning of the boom.

### 2.4.3 Heterogeneity in the Contagion Effect

We test for heterogeneity in the average contagion effect along a number of dimensions. The first is distance. We already found that only the nearest neighbor matters. A natural extension is to ask whether the strength of the contagion impact associated with the closest neighbor increases with its proximity to its focal market. The first two columns of Table 2.4 show that the answer is no. Those figures are the output from a regression like that in equation (2.3) that further interacts the neighbor's relative year dummies with an indicator for whether that neighbor is more or less than the median distance of about 40 miles away from the focal market. Although the estimates tend to be imprecise, the point estimates show little difference between relatively close and farther away nearest neighbors, especially around the time when the neighbor's boom begins. Thus, contagion effects arise only from the nearest neighbor, but they do not vary materially based on how close that nearest neighbor is.<sup>36</sup>

It also seems natural to ask whether contagion impacts depend upon the relative sizes of the focal and neighbor markets. To investigate this, we classified focal MSAs whose population sizes are within  $\pm 50\%$  of the population of the closest neighbor as being of similar size. They are considered larger if they have at least 50% more population, and smaller if they have less than 50% of the population of the closest

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<sup>36</sup>The interquartile range of distances between neighboring markets runs from 30-48 miles, so there is not much variation for much of the sample. The mean is larger at 74 miles, but that reflects the influence of Honolulu, whose nearest neighbor is over 2,000 miles away. The next biggest distance is 111 miles. We also experimented with alternative groupings such as dividing markets into whether their nearest neighbor was less than 30 miles away, from 36-60 miles away, and greater than 60 miles away. The results were no different from those reported here.

neighbor. Appendix Figure 2.A shows the distribution of relative population size, and the thresholds used to determine groups of MSAs for both geographic and economic neighbors. The estimates reported in the third and fourth columns of Table 2.4 indicate that the contagion effect on a focal market is larger if the nearest neighbor is substantially bigger than the focal area. Prices in the focal market are 1.3% higher immediately when the large near neighbor booms, but are little changed when the focal market is bigger (row 3 for Relative Year [1]). Prices stay higher in subsequent years for focal markets being influenced by large neighbors. In sum, size does matter and in an intuitive way in the sense that contagion effects are much larger (and consistently statistically significant) if the nearest neighbor is large relative to the focal market.

The final dimension along which we investigated whether there is any heterogeneity in contagion effects is by the degree of the focal market’s elasticity of housing supply. For this test, we split the focal MSAs into three groups according to the supply elasticities provided by Saiz (2010).<sup>37</sup> Results are reported in the final two columns of Table 2.4 for the bottom third (supply inelastic) and top third (supply elastic) groups. Note that prices do not jump when the closest neighbor of an inelastically supplied market begins to boom. However, for the most elastically-supplied metros, prices in the focal market are about 2% higher if its nearest neighbor begins to boom. The gap increases to about 3% by the third year of the boom. Performing calculations analogous to those discussed above for the economic importance of the average contagion effect show that spillovers could account for nearly two-thirds of the jump in prices at the beginning of booms in the most elastically supplied markets. Basic economics suggests that any contagion effects would be more likely to be capitalized in the inelastically supplied markets, *ceteris paribus*, so this outcome may seem counterintuitive at first glance. However, all else is not constant in this

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<sup>37</sup>Saiz (2010)’s supply elasticity estimates are available for only 76 of our metropolitan areas, so we start with a smaller sample for this particular analysis.

case. It turns out that a disproportionately large share of these markets are large coastal metropolitan areas with relatively small neighbors.<sup>38</sup> So, at least some of the variation we document by degree of supply elasticity could have been driven by the size effects just discussed.<sup>39</sup>

Clearly, no causal interpretation should be attached to this particular dimension of heterogeneity. However, when combined with previous results from Ferreira and Gyourko (2011), they help paint a more comprehensive picture of the beginning of the last boom across markets with different supply sides. For example, those authors report that a single own-market fundamental, the income of potential homebuyers in that area, jumped at the same time as price growth escalated in those markets, and did so by a magnitude large enough to account for virtually all of the increase in price appreciation at the beginning of the booms in the nation’s most inelastically supplied markets. If correct, that suggests there is little left to explain in terms of the beginning of booms in these places, which is consistent with our finding of little to no contagion effects there. In contrast, Ferreira and Gyourko (2011) also report that income did not jump contemporaneously with price growth at the beginning of booms in the most elastically-supplied markets, and therefore could not account for their booms. The absence of an own market fundamental driver in these places does

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<sup>38</sup>Included in this most inelastic tercile are the metropolitan areas of Barnstable Town, MA, Boston-Cambridge-Quincy, MA-NH, Bridgeport-Stamford-Norwalk, CT, Cleveland-Elyria-Mentor, OH, Deltona-Daytona Beach-Ormond Beach, FL, Eugene-Springfield, OR, Jacksonville, FL, Los Angeles-Long Beach-Santa Ana, CA, New Haven-Milford, CT, New York-Northern New Jersey-Long Island, NY-NJ-PA, Oxnard-Thousand Oaks-Ventura, CA, Palm Bay-Melbourne-Titusville, FL, Portland-Vancouver-Beaverton, OR-WA, Port St. Lucie-Fort Pierce, FL, Riverside-San Bernardino-Ontario, CA, San Diego-Carlsbad-San Marcos, CA, San Francisco-Oakland-Fremont, CA, Santa Rosa-Petaluma, CA, Sarasota-Bradenton-Venice, FL, Seattle-Tacoma-Bellevue, WA, Tampa-St. Petersburg-Clearwater, FL, and Vallejo-Fairfield, CA. Each of these areas also has one of the 99 markets in our sample as its closest market.

<sup>39</sup>We also investigated whether contagion effects varied by the relative timing of booms in order to see if contagion impacts occurred primarily when the focal market itself was booming. Appendix Figure 2.B shows a histogram of the difference between the timing of the boom of the focal MSA and its closest neighbor. There is wide variation in timing of the booms of these pairs of markets. We find that contagion effect seems concentrated in focal MSAs that were already booming when the closest neighbor started to boom. This result suggests salience being a feature of the contagion effect. But as with the heterogeneity by supply elasticity, relative timing is correlated with other factors, such as market size.

not necessarily imply a role for contagion, but it shows there is room for it. And, we find that it played a significant role.<sup>40</sup>

#### 2.4.4 Mechanisms

In this subsection, we begin to investigate some of the potential mechanisms that could account for the influence of a near neighbor’s boom on house prices in the focal market. We are particularly interested in whether our contagion effects are fundamentally-based in the sense discussed in Section 2.2. If not, the relevance of our results for policy makers is increased, as they well may want to reevaluate their past practice of not intervening in response to asset booms in housing markets if their spread is based on some type of irrational exuberance or otherwise mistaken expectations.

We begin by asking whether there are visible economic changes in the focal market that may be driven by the neighboring market boom. We then alter our baseline specification to account for these potential fundamental drivers. Four fundamentals are investigated, with each having received prominent mention in previous academic research or by policy makers and the popular press. They are focal market income, mortgage market activity, net migration flows into the focal market and the share of house ‘flips’ in overall market sales. Table 2.5 reports results using these local market traits as the dependent variable in a specification similar to that in equation (2.3), with one difference being that here we include MSA fixed effects rather than own price lags.

Column 1 reports estimates for focal market income, where income is defined as

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<sup>40</sup>The markets in the most elastic tercile which also have one of our 99 markets as their closest neighbor include mostly smaller metropolitan areas in the interior of the country and off the west coast of California. These include Cincinnati-Middletown, OH-KY-IN, Columbus, OH, Corvallis, OR, Dayton, OH, Flagstaff, AZ, Fresno, CA, Gainesville, FL, Merced, CA, Modesto, CA, Pittsfield, CA, Redding, CA, Stockton, CA, Visalia-Porterville, CA, and Yuba City-Marysville, CA. These smaller markets tend to have bigger neighbors. And, note that this list does not include big Sunbelt housing bubble markets such as Phoenix and Las Vegas (which are in the middle tercile and which do not have significant contagion effects).

the average income for all mortgage applicants in that market and quarter. If what is driving our contagion result is a real spillover such as focal market income going up because of boom in a nearby market, then we should see it changing along the timeline of the neighbor’s boom. Table 2.5’s results show that there is a jump from zero to 1% in Relative Year [1], but this impact is not precisely estimated (the  $t$ -statistic is 1.3). The results are similar for the second and third relative years. It is only at least four years after the nearest neighbor booms that we see focal market income higher by a statistically significant amount. One would not want to interpret these coefficients as proving that contagion does not operate via spillovers onto focal market income, but they also provide no robust evidence to the contrary.

The next two columns investigate whether the contagion effect might operate through credit markets in some fashion. We approach this question by examining two aspects of credit lender activities. First, we investigate whether the mortgage lender bases become more similar during and after the nearest neighbor’s boom. The intuition is that if lenders observe a boom in that neighbor, they might increase their activity in the focal market for reasons just discussed in Section 2.2. We use a proportional index to measure lender similarity.<sup>41</sup> Second, we investigate whether lenders speed up mortgage lending during and after the closest neighbor booms by calculating the rate at which each lender increases mortgage issuance in the focal market.

The regression results in Column 2 and 3 indicate that on average, both lender similarity and lending amount largely are unaffected by the housing boom of the nearest neighbor.<sup>42</sup> Lending amount by subprime lenders also does not respond to the neighbor’s boom. If we restrict the analysis to the top 5 lenders with the highest

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<sup>41</sup>This lender similarity index is calculated as  $1 - \frac{1}{2} \sum_{l=1}^L |m_l - n_l|$ , where  $m$  and  $n$  are the market shares of lender  $l$  in the focal and the nearest neighbor markets, respectively. A value of zero implies no similarity, while a value of one means that each lender has the same shares in both markets.

<sup>42</sup>This is one case in which classifying neighbors based on economic rather than geographic distance matters. Lender similarity increases by 1.3 percentage points by relative year three when we use migration flows to order neighbors.

lending amount for each market, we observe a jump in lender similarity index in Relative Year [1]. However, this result is not robust when we go from top 5 to top 10 or top 3 lenders. Including only top lenders also leads to a jump in lending growth rate in Relative Year [2] (the coefficient is around 0.5), but not in Relative Year [1]. However, lending growth also is higher before neighbor’s boom, with coefficients on Relative Year [-1] and Relative Year [-2] being in the range of 0.2-0.4. Given this noticeable pre-trend, it is difficult to come to any conclusion that major credit lenders are the channel in disseminating positive housing market shocks. Therefore, based on our two measures of lender activities, we did not find robust and material role of lenders in causing contagion.<sup>43</sup>

The fourth and fifth columns report analogous specifications using net migration between two MSAs (which uses IRS data on annual tax records) and focal market flippers (based on the fraction of transactions conducted by speculators) as the dependent variable. Both sets of results show no discernible effect before or after the beginning of the nearest neighbor’s housing boom.<sup>44</sup>

The fact that only focal market income shows any correlation with the time line of the nearest neighbor’s boom, especially its beginning, indicates that these fundamental factors are unlikely to be able to account for our estimated contagion elasticity. Table 2.6 provides additional support for this conclusion with alternative specifications similar to our baseline equation (2.3) that use the logarithm of focal market

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<sup>43</sup>Nonetheless, this average result does not exclude the possibility of major credit lenders responding to neighbor’s booms in other dimensions. The increase in similarity index among top lenders and the dip in their lending amount right at neighbor’s boom may suggest some bank-level spillovers or substitution effects going on across markets. To fully understand the role of credit markets in this contagion context would require a closer examination of a full spectrum of lender behaviors, including those at the corporate level (e.g., mergers and acquisitions, and shocks to other business sectors of lenders), which is beyond the scope of this paper.

<sup>44</sup>We also investigated the shares of experienced and inexperienced speculators, respectively. Speculators are defined as experienced if having flipped at least four homes during the sample period, or inexperienced if otherwise. We did not find significant jumps in either experienced or inexperienced flippers when the closest neighbor begins its boom. However, the market share of experienced flippers declined by up to 0.5 percentage point (which is statistically significant) since two years after the closest neighbor booms, which is in line with those types of flippers being more sophisticated in timing the housing cycle and maximizing their return (Bayer et al., 2011).

price as the dependent variable. The first column reports results from a model that includes focal market fundamental controls and the log average price of near neighbors on the right-hand side, in addition to the standard controls from the baseline specification in Table 2.2. Our fundamental controls are local incomes, migration, subprime and FHA lending market shares, percentage of speculative buyers, percent minority, average loan-to-value, average square footage, and the local unemployment rate. Note that these new estimates are very similar to the baseline results presented in Table 2.2. Controlling for these fundamentals does not change the magnitudes or time pattern of estimated contagion effects very much, indicating that the spatial spillovers are not being transmitted via the fundamentals that we consider here.

Thus far, we have abstracted from expectations of future fundamental factors, effectively treating actors as myopic. The second column of each panel in Table 2.6 begins to address this issue by adding four leads of own market income to the previous specification.<sup>45</sup> Effectively, this presumes that local residents can fully predict the path of local incomes over the next four quarters. The inclusion of such stylized expectations does not change the estimated contagion effect. The third and final column of each panel reports results from adding four quarterly leads of all fundamentals, not just income. Once again, the magnitudes of the point estimates as well as the time pattern are relatively unchanged.

That we find no evidence that these spatial spillovers work through fundamental factors has potentially important implications for how policy makers should view intervention during housing booms. To the extent the booms are spread by non-fundamental forces (e.g., some type of Keynesian/Shillerian irrational exuberance), they might want to try to stop them from growing in scale and scope. Of course, fundamentals could encompass more than just income growth (or speculators, migration, etc.), and we would like to control for expectations as generally as possible. One

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<sup>45</sup>We report results for including leads of only this fundamental separately because it was the only one that exhibited any positive correlation with the time line of the closest neighbor's boom.



extreme way that does eliminate the impact of contagion is to presume that the future path of price growth in the focal market is known with certainty in every period. In this context, the effect of a nearby housing boom on future prices in the focal MSA is immediately known and expectations are simply equal to the future value of price growth in the focal area. Adding four quarterly price growth leads completely wipes out the contagion impact. This indicates that the spillover could be operating primarily via expectations. Unless those expectations are based solely on fundamentals, the implications just discussed still hold. The likelihood that contagion operates via effects on expectations makes that issue an essential component of future research, but that clearly is beyond the scope of the current paper.

## **2.5 Additional Analysis: Instrumental Variable, Extensive Margin and Housing Bust**

### **2.5.1 Alternative Model and Instrumental Variable**

In this subsection, we relate price changes in the focal MSA with price changes from all neighbors, not just the nearest two. This direct estimation of the contagion elasticity has the benefit of allowing us to use an instrumental variable strategy to deal with omitted factors. Also, by interacting the price changes of the closest neighbor with a set of relative year dummies, we are able to explore how this effect varies over the course of a neighbor market's housing boom. The downside of this approach is that the specification does not allow us to fully observe the dynamic pattern of contagion, as we restrict the effect of neighboring price changes to operate through only one quarterly lag.

More specifically, we group neighboring locations into bins based on their distance from the focal market and estimate the following equation:

$$\begin{aligned}
\Delta P_{m,d,t} = & \sum_{r=-4, r \neq 0}^4 \rho_r^1 \cdot \psi_{m,r,t}^1 \cdot \Delta \bar{P}_{m,t-1}^1 + \sum_{k=2}^K \rho^k \cdot \Delta \bar{P}_{m,t-1}^k + \sum_{\tau=1}^4 \rho_\tau \cdot \Delta P_{m,d,t-\tau} \\
& + \sum_{r=-4, r \neq 0}^4 \rho_r \cdot \psi_{m,r,t} + \gamma_{d,t} + \xi_{m,d,t}
\end{aligned} \tag{2.4}$$

where  $\Delta \bar{P}_{m,t-1}^k$  is the lagged average price growth among neighboring MSAs falling into bin  $k$  for focal MSA  $m$ . In theory, we could allow each neighbor to be in its own bin based on how close it was to the focal area. However, that turns out not to be practical due to data limitations, so we bin neighbors based on distance rankings 1, 2, 3-5, 6-10, 11-50, and 51+.<sup>46</sup> This makes the coefficients,  $\rho^k$ , the elasticity of focal area current price growth with respect to the *average* of lagged price growth among neighbors in bin  $k$ . For the closest neighbor, we further interact the lagged price growth variable with relative years to that neighbor's boom – resulting in a coefficient,  $\rho_r^1$ , for each of the closest neighbor's relative years, as shown in the first term on the right-hand side of (2.4). Relative year [0] again is the omitted category in all specifications. Thus, the coefficients on the lagged average price growth of the closest neighbor are interpreted relative to the effect in the 12-month period prior to that neighbor's boom.

One concern is that, even after including lags of the dependent variable (the third term on the right-hand side of (2.4)) and area-by-time fixed effects (the  $\gamma_{d,t}$  vector), there still could be some common omitted factors helping drive the observed correlations. Ideally, we would like an instrumental variable that shifts the lagged average price growth of the focal MSA's closest neighbor, but does not directly affect the contemporaneous appreciation rate in the focal market itself. If taken literally,

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<sup>46</sup>As in our main regressions, we include an indicator for whether we have price data for a given bin and interact this indicator with the relevant lagged average price variable. Results are qualitatively similar when we drop MSAs with missing neighbors' price data and also when we calculate distances using only the 99 MSAs for which we have data.

our estimating equation implies that further lags of the neighbor’s price growth could potentially serve as an instrument because those variables would only affect the focal market’s contemporaneous price growth through their impact on the lagged neighbor’s price growth. This leads us to instrument for the lagged average price growth in each group of neighbors using one further lag of the average price growth among the relevant neighboring areas.<sup>47</sup>

Table 2.7 reports the results of estimating equation (2.4). Once again, there is a clear pattern that shows a shift in the importance of contagion right after the first year that close neighbors boom. Estimate for the first year of the closest neighbor’s boom is 0.148, or approximately half the elasticity derived from our baseline estimates when considering the first year of the housing boom. Estimates for subsequent relative years fade relatively slowly.

## 2.5.2 A Hazard Model of Housing Boom Contagion

Our work above focuses entirely on the magnitude of contagion. In this section, we give empirical content to the extensive margin on the timing of booms that was suggested by Figure 2.1. We estimate simple hazard models to see if the probability of a focal market booming is influenced by the fact that neighbors boomed previously, after controlling for a host of covariates that also might account for the beginning of a boom.

Recent work on technology diffusion provides an intuitive way to generate contagion that is particularly appropriate for the specifications estimated in this subsection. In some of that research, contagion refers to a process in which people adopt a new technology when they physically meet with others who have already adopted it

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<sup>47</sup>It turns out that the second quarterly lag of neighbor’s price growth has a small and statistically insignificant impact on the focal MSA price appreciation, after controlling for all other covariates. In the specifications that interact the lagged average price growth of the neighbors with the focal MSA relative year indicators, we also interact lagged neighbors income growth and the second lag of neighbor’s price growth with the focal area’s relative year and include the full set of these interactions as instruments.

(Young (2009); Comin et al. (2012)). In our context of housing booms, this suggests that the probability of one metropolitan area entering a housing boom increases with its connection to other areas that already have boomed. A standard assumption is that the intensity of the connection decays in distance between MSA pairs at a constant rate. Given that assumption, it is straightforward to generate the following two conclusions from a simple model: (a) if MSA  $q$  enters a housing boom at time  $t$ , that increases the hazard of MSA  $m$  ( $m \neq q$ ) having a housing boom at time  $t + 1$ ; and (b) this contagious effect is larger the closer is MSA  $m$  to MSA  $q$ .<sup>48</sup>

To investigate these implications, we begin by estimating whether the beginning of booms in neighboring MSAs affects the hazard of the focal MSA entering a housing boom. We consider the following proportional hazard model relating the hazard of each focal MSA entering a boom in quarter  $t$ ,  $h_m(t)$ , to a series of factors as noted in the following equation:

$$h_m(t) = h_0(t) \exp \left\{ \sum_{k=1}^K \gamma_k \sum_{j \in S_m^k} \text{Boom}_{j,t-1} + \Delta X_{m,t} \beta + \sum_{\tau=1}^4 \theta_\tau \Delta P_{m,t-\tau} + \eta \text{PctBoomed}_{t-1} + \delta_R \right\} \quad (2.5)$$

The primary coefficients of interest are the  $\gamma_k$ 's on variable  $\sum_{j \in N_i^k} \text{Boom}_{j,t-1}$ , with the latter reflecting the number of MSAs among neighbors in bin  $k$  that began their boom in the previous quarter. We allow for multiple groups of neighbors (indexed by  $k$ ) based on how close they are to the focal MSA. As before, we rank each focal MSA's neighbors and group them into  $K$  mutually exclusive bins based on these ranks, with  $S_m^k$  denoting the set of neighbors in bin  $k$  for focal MSA  $m$ . A positive coefficient  $\gamma_k$  would suggest a positive contagion effect. If the contagion effect decays with distance,

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<sup>48</sup>See Appendix 2.7.2 for a more formal presentation of how that approach generates specifications of the type we estimate in this section.

we should also expect the  $\gamma_k$ 's of closer neighbors to be larger than those of farther away neighbors. As with the estimation of magnitudes, we control for many potential correlates of a housing boom through vector  $\Delta X_{m,t}$ .

We use the same bins as in Table 2.7. Table 2.8 then lists summary statistics on the number of booms started across the different bins of neighbors. The very small means for the bins containing five or fewer markets document how unlikely it is that even a single boom began in any given quarter among the few markets in those bins. Thus, the lagged value of this variable ( $\sum_{j \in S_m^k} \text{Boom}_{j,t-1}$ ) will contain many zeroes, making it difficult to estimate precise contagion effects about the timing of the beginning of local market booms. Nevertheless, we use these bins initially given the differential importance of the two closest neighbors in the contagion magnitude results and discuss findings later that use larger bin sizes.

Our baseline results are from a common parametric specification, the exponential model, which assumes a flat baseline hazard  $h_0(t) = \exp(\alpha_0)$ . Implied changes in the probability of the focal area experiencing a boom this quarter (i.e., the hazard ratio) from one more neighbor having experienced a boom last quarter are reported in Table 2.9. The unconditional hazard, which is reported in column 1, indicates that the probability of the focal MSA booming this quarter more than triples if one more of its two physically closest neighbors boomed last quarter. The coefficients on the next three bins (Neighbors[3-5], Neighbors[6-10], and Neighbors[11-50]) are much smaller and none is statistically significant at conventional levels. That the lack of significance might be due to the nature of these variables having so little variation is indicated by the statistically significant impact of the group of furthest away markets (Neighbors[51+]). Still, it is lagged booms among the two closest neighbors that have the strongest correlation with a contemporary boom in the focal market.

Controlling for a standard set of covariates lowers the estimated effect substantially, as reported in column 2. This time, the coefficients on the bins for all but the

two closest neighbors are close to or below 1, indicating they have no positive impact on the probability of the focal market booming this period. And, the coefficient on the bin for the two closest neighbors falls by more than half. Even the 49% increase in probability implied by the coefficients on Neighbors[1-2] is not statistically significant, although that could be due to the nature of our data as discussed above. This conclusion is supported by the findings reported in column 3, which uses a Weibull hazard.<sup>49</sup> It reports a statistically significant correlation between lagged booms in very near neighbors and contemporaneous booms in the focal markets. These results show that if one of the two closest neighbors boomed last quarter, the probability of the focal market booming this quarter is about 70%-80% higher. The standardized marginal effect is smaller, of course, given the 0.18 standard deviation on Neighbors[1-2]. Using the middle of the range estimate from the Weibull hazard model, it is about 30% (i.e.,  $1.69 \times 0.18$ ).

These results only allow for a contagion effect from a single quarterly lag of neighbors' booms. We also estimated models that allowed for an increase in the number of booming neighbors over the past 6 to 12 months. There is a modest increase in the hazard (to 32%) if we allow for booms in any of the previous two quarters, but the result does not increase further if we allow for booms in any of the previous four quarters. Thus, the spatial effect estimated here appears to happen fairly quickly. This is also consistent with the elasticity estimates reported above.

The magnitude of the economic impact is hard to gauge on its own. We can gain some useful perspectives by comparing it to the impacts on the hazard ratio of standard deviation changes in other variables. Among the underlying controls, the focal market's current income growth and previous quarter's price growth also were highly statistically significant predictors of a higher probability of booming. A one standard deviation higher own income growth rate is associated with about a 28%

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<sup>49</sup>A Weibull hazard model presumes a monotonic baseline hazard  $h_0(t) = pt^{p-1} \exp(\alpha_0)$ . We experimented with different functional forms to see if the pattern of results was materially affected.

higher probability of the focal market booming. A one standard deviation higher rate of lagged house price appreciation is much more influential, as it is associated with an 87% higher hazard ratio. Thus, the standardized marginal effect of a boom in a very close neighbor appears to be quite influential and on a par with a standard deviation increase in its own income growth rate.<sup>50</sup> And, that we still find a meaningful influence of lagged near neighbors after controlling for everything else shows that the implications of the ‘eyeball econometrics’ from Figure 2.1 are not entirely due to many other potentially important factors.<sup>51</sup>

### 2.5.3 Was There Contagion During the Spread of the Housing Bust?

Our results indicate that contagion played a statistically and economically meaningful role in the timing and magnitude of the spread of housing booms across metropolitan areas. For completeness, we also explored the extent to which the same is true for the bust. In many respects, analysis of the bust is more challenging. One of the challenges is in deciding how bust is defined and determined. We choose it to be the quarter in which nominal house prices peaked in the relevant MSA. While that may be intuitive, it also is much more *ad hoc* than our definition of the beginning of the boom, which is based on an external prediction of an economic model. Another challenge is that the bust was much more temporally correlated across markets than was the boom. This can be seen in Figure 2.3 which plots histograms of the quarters

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<sup>50</sup>The growth rate in the percentage of buyers with mortgages insured by the FHA also is a very powerful control. As expected, it is associated with a lower probability of booming. Specifically, a one standard deviation increase in that share is associated with an 86% fall in the hazard. Other statistically significant controls include the growth rate of the metropolitan area unemployment rate, as well as the second and fourth quarterly lags of focal market house price appreciation.

<sup>51</sup>Conditional hazard model estimates using more aggregate bins (Neighbors[1-10], Neighbors[11-50] and Neighbors[51+]) do not show any impact on the timing of the beginning of the boom in focal markets. Averaging across the ten closest neighbors masks the distinct impact of the two closest neighbors. As in the analysis of the magnitude of contagion, only the closest neighbors appear to matter.

in which local market booms and busts began. This plot includes all markets that had a statistically significant boom. Note that the temporal concentration of busts is much greater, with every market experiencing a peak in prices within a 2.5-3 year period from mid-2005 through early 2008.

Figure 2.4 then provides more geographical detail with its plots of market busts over time. The first panel in this figure is identical to the first panel in Figure 2.1 and plots the MSAs for which we never estimate a statistically significant boom. The remaining panels show the timing of the bust among those MSAs that experienced booms. Unlike Figure 2.1's plots of the time line of metropolitan area booms, we see markets in all parts of the country, not just in coastal California and upper New England, with early price peaks between 2003-2005. The largest fraction of those peaks happening in the last two quarters of 2005. Thus, the beginning of the bust is more national in scope than was the beginning of the boom. The subsequent plots in this figure do show a spreading out of the 'busts' to nearby markets. In the west, prices tended to peak earlier in interior markets and then spread to the coast. In Florida, the first price peaks were in markets on both coasts of that state. Peaking then occurred in a few other coastal markets before spreading to interior markets.

While this has the flavor of contagion seen in the start of the boom, more detailed analysis shows this not to be the case. For example, Table 2.10 reports hazard model estimates akin to those in Table 2.9, except here the dependent variable is the start of the bust, not the start of the boom. Unconditionally, lagged busts of neighbors are positively correlated with contemporary busts in focal markets, and near neighbors matter the most (column 1). However, column 2 of this table shows this conclusion of 'eyeball econometrics' from Figure 2.4 does not survive the inclusion of covariates.<sup>52</sup>

In Table 2.11 we estimated price specifications for the bust akin to those in Table 2.2. First, using geographic distance, we do not see significant jumps in the magnitude

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<sup>52</sup>All markets are used in this particular estimation, including those that did not boom. The results are virtually identical if we restrict the sample to those that did boom.



of the contagion effect in Relative Year [1]. But focal prices decline by 1.2% and 3.8% in relative years two and three respectively. Results using economic distance follow a similar pattern. When compared to the magnitude of the price decline in the neighboring MSA, we find an elasticity of around 0.15. Those results are surprisingly similar to the ones observed during the spread of the housing boom.

## 2.6 Conclusions

We provide estimates of the role of contagion in the most recent American housing boom and bust. We find a statistically and economically meaningful role for contagion during the beginning and spread of the housing boom, and mixed evidence of contagion for the bust. Our key results are as follows. First, contagion impacts arise only from the very closest neighbors. There is no evidence of spillovers associated with more distant neighbors. The elasticity of focal market prices with respect to changes in its nearest neighbor's prices is in the range of 0.10-0.27. Back-of-the-envelope calculations suggest this is large enough to account for up to 30% of the jump in prices at the beginning of local booms, on average.

Second, they occur primarily in the most elastically-supplied markets and do not appear to be relevant in inelastically-supplied areas. The economic importance of spillovers is larger for the subset of the most elastically-supplied markets, accounting for up to 60% of the beginning of their booms. Considered in tandem with findings reported in Ferreira and Gyourko (2011), these results help paint a more comprehensive picture of the beginning of the last boom in housing. One key fundamental, own market income, can account for almost all of the initial jump in prices in the nation's most inelastically-supplied metropolitan areas. Contagion appears to have played little to no role in these places. In contrast, no fundamental change appears to be correlated with the start of booms in the most elastically-supplied housing markets,

but contagion from neighbors looks to have played an important role. Contagion impacts are also greater when being transmitted from a larger to a smaller market.

Finally, we found that local fundamentals and expectations of future fundamentals have very limited ability to account for our estimated contagion effect. That contagion transmission is not associated with local fundamentals suggests a potential role for non-rational forces. That is an issue in urgent need of new research because, if contagion does reflect some type of irrational exuberance, policy makers may want to rethink their past policy of not intervening to stop the spread of asset booms in housing.

## 2.7 Appendix

### 2.7.1 Estimating Multiple Break Points

In estimating the break points, we allow for the possibility that a given market might experience more than one housing boom during the course of our sample period. Our method is recursive in that we first test for the existence of one break point against the null hypothesis of zero. Given the existence of at least one break point, we can then test the hypothesis of  $m + 1$  break points against the null of  $m$  using the results from Bai (1999). Bai and Perron (1998) show that the test for one break is consistent in the presence of multiple breaks, which is what allows for this sequential estimation procedure.

More specifically, let  $0 < \phi_1 < \dots < \phi_m < 1$  mark the proportions of the sample generated by the  $m$  break points estimated under the null hypothesis. For technical reasons, we require that  $\phi_i - \phi_{i-1} > \pi_0$  for some small  $\pi_0$  where we define  $\phi_0 = 0$ ,  $\phi_{m+1} = 1$ . Further, let  $\eta_i = \frac{\pi_0}{\phi_i - \phi_{i-1}}$ ,  $i = 1, \dots, m + 1$ . The likelihood ratio test compares the maximum of the likelihood ratio obtained when allowing for  $m + 1$  breaks to that from only allowing for  $m$ . The distribution of this likelihood ratio

statistic is given by:

$$P(LR > c) = 1 - \prod_{i=1}^{m+1} \left( 1 - P(\sup_{\pi \in [\eta_i, 1-\eta_i]} Q_1(\pi) > c) \right) \quad (2.6)$$

which we calculate by recursive application of the method provided in Estrella (2003).

We apply this procedure to test for the existence of two break points against the null of one as well as three against the null of only two among those MSAs for which we find at least two statistically significant break points. There are some noteworthy practical issues involved with carrying out this procedure. We have not until this point said where the sample proportions  $\pi_0$ ,  $\pi_1$ ,  $\pi_2$  come from. In practice, we restrict the full sample period for each MSA to lie between the first quarter in the data and the peak of price growth. We then do not allow any break points to lie in either the first or last two quarters of this sample for each MSA. This determines the fractions  $\pi_1$  and  $\pi_2$  which, because different MSAs have a different number of quarters, will vary across areas.

When estimating multiple break points, we further require that any two break points be at least four quarters apart. This determines the fraction  $\pi_0$  which, again, will vary across areas due to differing sample sizes. Because of these restrictions, we are not able calculate p-values for many MSAs in the case of multiple breaks. The reason for this can be seen from the expression in (2.6). Because this expression requires that  $\eta_i < 0.5$ , we must impose that  $\frac{\pi_0}{\phi_i - \phi_{i-1}} > 0.5$  for all  $i$ . This implies that we will not be able to calculate p-values for the two-break case in MSAs (neighborhoods) where the first break is less than  $\pi_0/0.5$  from the beginning of the sample period. Naturally, this restriction is more burdensome when trying to calculate p-values in the three break case.

### 2.7.2 A Hazard Model of Housing Boom Contagion

Consider an economy with  $N$  metro areas. The probability of MSA  $m$  entering a housing boom increases if it “meets” other MSAs which already have entered a housing boom. Assume  $\alpha$  is the frequency of such a meeting and that the connection between MSA pairs decays in distance at rate  $\delta$ . Following Comin, et al. (2012), we can write MSA  $m$ ’s probability of not entering a housing boom at time  $t + h$  conditional on not having a housing boom in time  $t$  as:

$$P(0, m, t + h) = P(0, m, t) \left[ \frac{\sum_{q \neq m} P(0, q, t) e^{-\delta r_{mq}}}{\sum_{q \neq m} e^{-\delta r_{mq}}} \right]^{\alpha h} \quad (2.7)$$

where  $r_{mq}$  denotes the distance between MSA  $m$  and MSA  $q$ . Taking  $h \rightarrow 0$ , we have

$$\frac{\partial \ln P(0, m, t)}{\partial t} = \alpha \ln \left( \sum_{q \neq m} P(0, q, t) e^{-\delta r_{mq}} \right) - \alpha \ln \left( \sum_{q \neq m} e^{-\delta r_{mq}} \right) \quad (2.8)$$

By assumption,  $P(0, q, t) = 0, \forall t \leq \tau$  if MSA  $q$  enters a housing boom at time  $\tau$ . As long as some MSA enters a boom, equation (2.8) implies that  $\frac{\partial \ln P(0, m, t)}{\partial t} < 0$ , so that the hazard of entering a housing boom increases over time. To consider the contagious effect of housing booms, suppose MSA  $q$  booms at time  $t$ . That increases the hazard of MSA  $m$  having a boom because

$$\frac{\partial \ln P(0, m, t)}{\partial t \partial P(0, q, t)} = \frac{\alpha e^{-\delta r_{mq}}}{\sum_{q \neq m} P(0, q, t) e^{-\delta r_{mq}}} > 0$$

In addition, the contagious effect of a housing boom in MSA  $q$  decreases over geographical distance, because

$$\frac{\partial^2 \ln P(0, m, t)}{\partial t \partial r_{mq}} = \frac{\alpha \delta e^{-\delta r_{mq}}}{\sum_{q \neq m} e^{-\delta r_{mq}}} > 0$$

In sum, this framework provides two relevant implications:

**Implication 1:** The fact that MSA  $q$  enters a housing boom at time  $t$  increases the hazard of MSA  $m$  ( $m \neq q$ ) having a housing boom at time  $t + 1$ .

**Implication 2:** Contagion effect in *Implication 1* becomes larger when MSA  $q$  is closer to MSA  $m$ .

Figure 2.1: Timing of Housing Boom by MSA

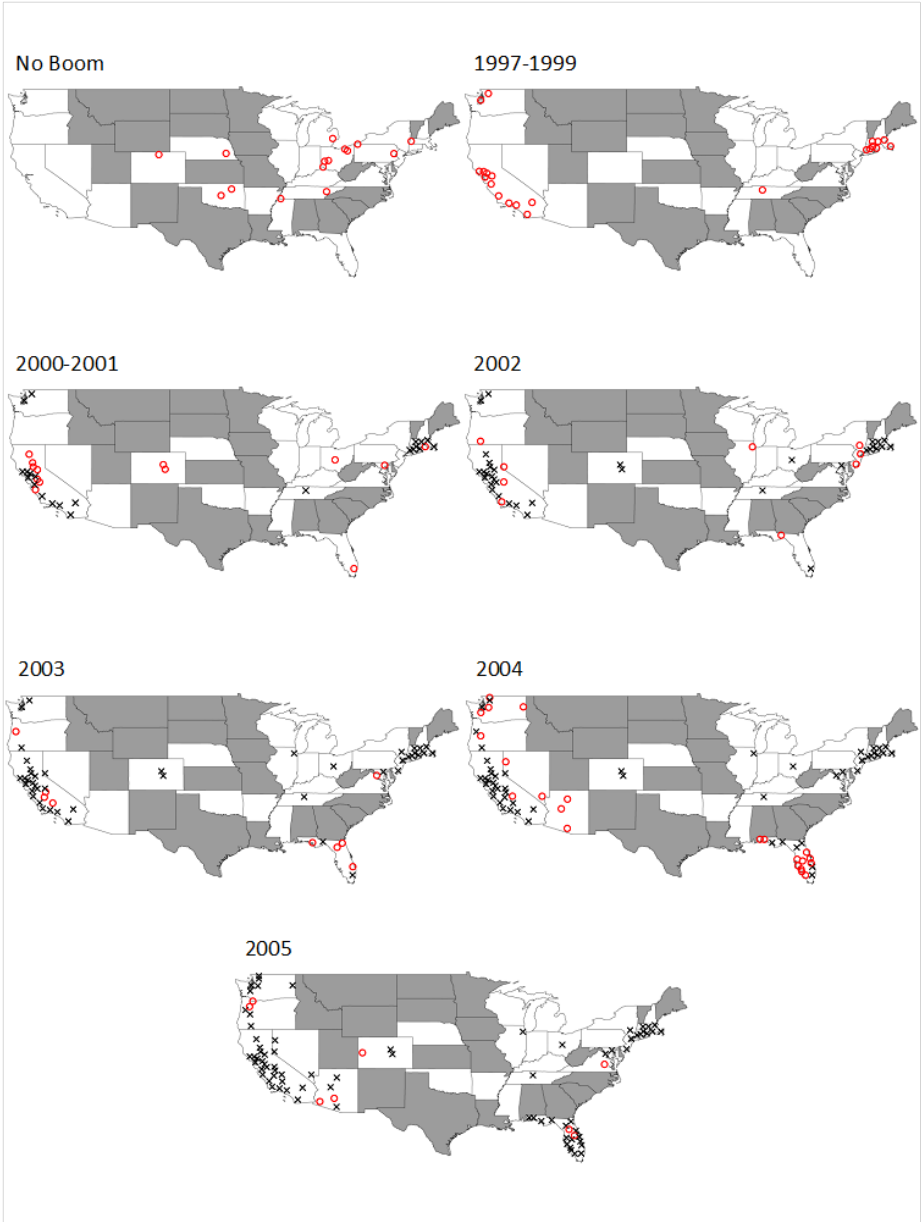
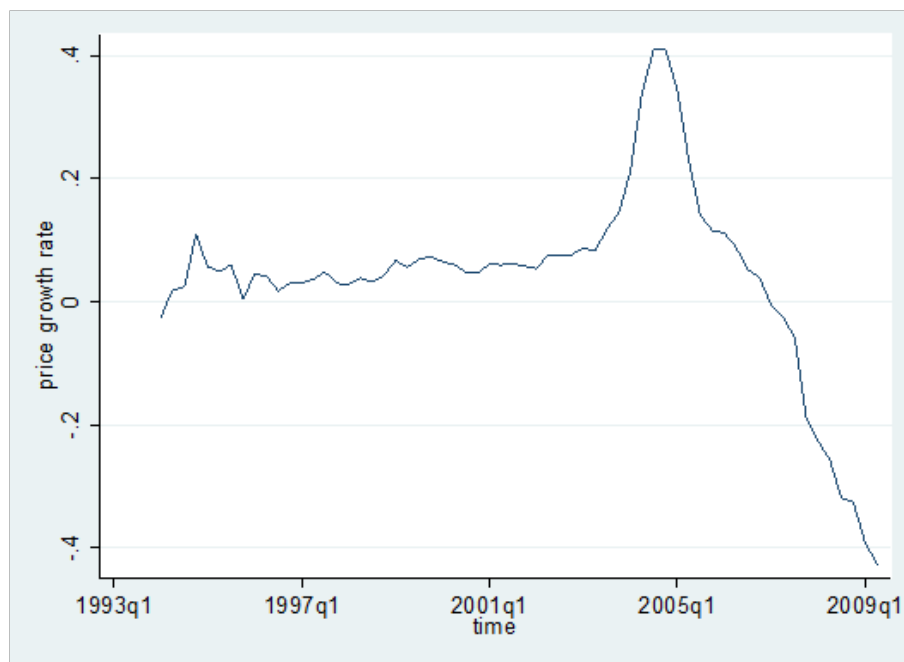


Figure 2.2: Las Vegas' Constant Growth Rate before Booming



Source: Ferreira and Gyourko (2011, Figure 2)

Figure 2.3: Histograms of the Beginning of Booms and Busts, MSAs

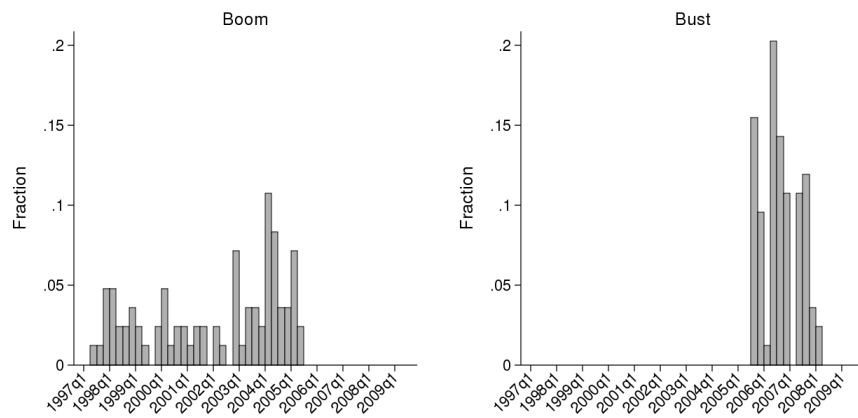




Figure 2.4: Timing of Housing Busts by MSA

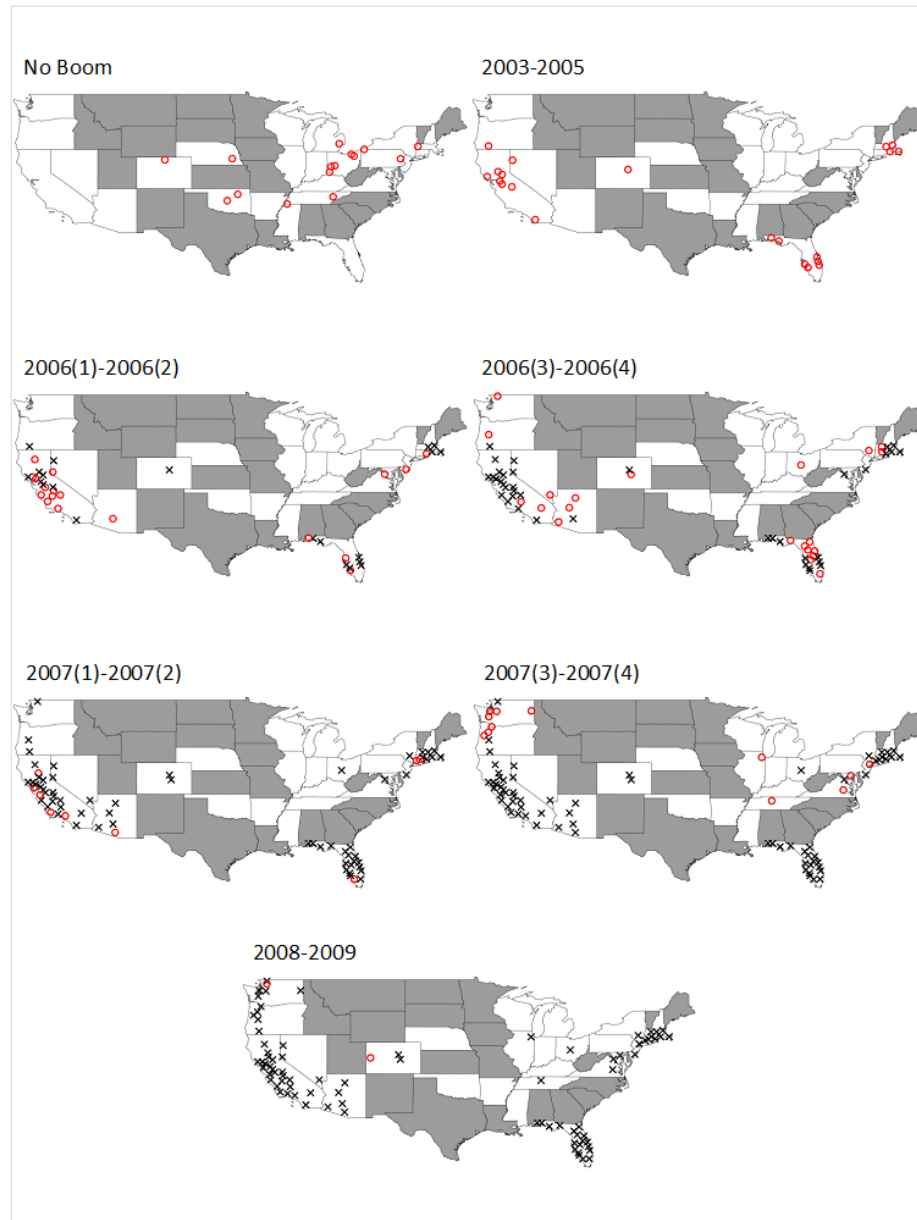


Table 2.1: Summary Statistics

	(1) Mean	(2) Std. Dev.	(3) 25 <sup>th</sup> Percentile	(4) 75 <sup>th</sup> Percentile
Price Index	132	52	96	156
Average Income (\$1000's)	75	26	56	86
Percent Minority	.18	.13	.075	.25
Percent Speculators	.054	.032	.031	.071
Percent FHA Insured	.096	.097	.014	.14
Percent Subprime Lenders	.14	.075	.087	.19
Average LTV	.73	.1	.69	.79
Average Square Footage	1672	161	1584	1764
Unemployment Rate	5.9	3.3	4	6.6
Net Migration	266	6878	-668	1408
N	5043			

Notes: Columns present descriptive statistics for all MSA-quarter observations in our sample. Observation counts in regressions will vary depending on the specification and control variables used.

Table 2.2: The Impact of Nearest Neighbor Housing Boom on Focal Market Price

<i>Dep. Var: log(Focal Market Price)</i>		
<i>Nearest Neighbor Relative Years</i>	Neighbor Boom Significant	Neighbor Boom Insignificant
Relative Year [-2]	-.003029 (.004662)	-.003357 (.01543)
Relative Year [-1]	-.000076 (.003184)	-.03365** (.01526)
Relative Year [1]	.008688** (.004191)	.00297 (.00563)
Relative Year [2]	.00977** (.004236)	-.02102* (.01091)
Relative Year [3]	.008542* (.004985)	-.01698*** (.005502)
Relative Year [ $\geq 4$ ]	.005247 (.004774)	-.006412 (.004954)
Quarter-by-Census Division FE		Y
Focal Market Relative Year FE		Y
Four Lags of Focal Log Price		Y
Neighbor-2 Relative Year FE		Y
N		4584

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic neighbor. Relative year [0] indicates the 12-month period preceding the boom of the neighboring MSA and is the omitted category. All specifications also include dummy variable(s) indicating whether the closest neighbor(s) are in the sample. Standard errors are clustered at the quarter by Census Division level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.3: The Impact of Nearest Neighbor Housing Boom on Nearest Neighbor Price

<i>Dep. Var: log(Nearest Neighbor's Price)</i>	
<i>Nearest Neighbor Relative Years</i>	(1)
Relative Year [-2]	.02815** (.01333)
Relative Year [-1]	.01751 (.0153)
Relative Year [1]	.03224 (.02066)
Relative Year [2]	.06701*** (.02199)
Relative Year [3]	.08197*** (.02188)
Relative Year [ $\geq 4$ ]	.1179*** (.01976)
Quarter-by-Census Division FE	Y
Focal Market Relative Year FE	Y
Four Lags of Focal Log Price	Y
Neighbor-2 Relative Year FE	Y
N	3583

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic neighbor. Relative year [0] indicates the 12-month period preceding the boom of the neighboring MSA and is the omitted category. All specifications also include dummy variable(s) indicating whether the closest neighbor(s) are in the sample. Standard errors are clustered at the quarter by Census Division level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.4: Heterogeneity in the Impact of Nearest Neighbor Housing Boom on Focal Market Price

<i>Dep. Var: log(Focal Market Price)</i>	Distance		Relative Size		Focal Market Elasticity	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Nearest Neighbor Relative Years</i>	$\leq 40$ miles	$> 40$ Miles	Focal Larger	Nbr. Larger	Inelastic	Elastic
Relative Year [-2]	-.004239 (.004747)	-.001194 (.005436)	-.001048 (.004448)	-.005334 (.005874)	-.001111 (.006739)	.001165 (.007681)
Relative Year [-1]	-.001823 (.003802)	.001363 (.004727)	-.001269 (.003668)	.000373 (.003878)	-.003162 (.004464)	.003788 (.006904)
Relative Year [1]	.008405 (.005087)	.008564 (.006187)	.00461 (.00417)	.01288** (.005703)	.00769 (.007545)	.01877* (.0103)
Relative Year [2]	.008409 (.005425)	.009215 (.006344)	.01116*** (.004208)	.009466* (.005423)	.008632 (.005739)	.02683** (.01069)
Relative Year [3]	.008389 (.007093)	.005968 (.005006)	.007858 (.005809)	.0104** (.004913)	.00978 (.007775)	.02897*** (.0102)
Relative Year [ $\geq 4$ ]	-5.5e-06 (.005004)	.0118** (.005328)	.005298 (.004551)	.008329 (.005509)	.01061* (.006347)	.01055 (.01232)
Quarter-by-Census Division FE		Y		Y		Y
Focal Market Relative Year FE		Y		Y		Y
Four Lags of Own Log Price		Y		Y		Y
Neighbor-2 Relative Year FE		Y		Y		Y
N	4647		4584		3586	

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic neighbor interacted with dummies for whether the focal market is in the category indicated in the column header for various measures of heterogeneity. Relative year 0 indicates the 12 month period preceding the boom of the neighboring MSA and is the omitted category. All specifications also include dummy variable(s) indicating whether the closest neighbor(s) are in the sample. Standard errors are clustered at the quarter by Census Division level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.5: The Impact of Nearest Neighbor Housing Boom on Focal Market Fundamentals

	(1)	(2)	(3)	(4)	(5)
<i>Nearest Neighbor Relative Years</i>	Focal Market Income	Lender Similarity	Lending Amount	Net Migration	Focal Market Flippers
Relative Year [-2]	-0.013 (.009)	.0127* (.0065)	0.0205 (0.0609)	84.43 (132.6)	.0078*** (0.0029)
Relative Year [-1]	-0.006 (.008)	0.005 (.0051)	0.0572 (0.0562)	91.49 (132.9)	0.0039 (0.0026)
Relative Year [1]	0.011 (.008)	0.0025 (.0039)	0.0239 (0.0505)	-43.23 (85.75)	-0.0004 (0.0032)
Relative Year [2]	0.016 (.0115)	-0.0015 (.0047)	0.0491 (0.0416)	-93.54 (78.01)	-0.0012 (0.003)
Relative Year [3]	0.0135 (.0132)	0.0004 (.0071)	-0.0230 (0.0448)	-165.4 (114)	-0.0038 (0.0031)
Relative Year [ $\geq 4$ ]	.0229* (.013)	0.0048 (.0073)	0.0194 (0.0618)	-251.6** (101.4)	-0.0037 (0.0032)
Quarter-by-Census Division FE	Y	Y	N	N	Y
Year-by-Census Division FE	N	N	Y	Y	N
MSA FE	Y	Y	Y	Y	Y
Focal Market Relative Year FE	Y	Y	Y	Y	Y
Neighbor-2 Relative Year FE	Y	Y	Y	Y	Y
Focal Market Price and LTV growth	N	N	Y	N	N
N	4877	4219	996941	1215	4829

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic neighbor. Relative year [0] indicates the 12-month period preceding the boom of the neighboring MSA and is the omitted category. All specifications also include dummy variables indicating whether the closest neighbor is in the sample. Standard errors are clustered at the MSA level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.6: The Impact of Nearest Neighbor Housing Boom on Focal Market Price, Controlling for Local Fundamentals and Expectations

<i>Dep. Var: log(Focal Market Price)</i>			
<i>Nearest Neighbor Relative Years</i>	(1)	(2)	(3)
Relative Year [-2]	-.003202 (.004133)	-.003899 (.003979)	-.002159 (.004018)
Relative Year [-1]	-.000332 (.003053)	-.000577 (.003016)	.000934 (.00288)
Relative Year [1]	.007821** (.003796)	.008673** (.003624)	.007878** (.003235)
Relative Year [2]	.008964** (.004278)	.01033** (.004157)	.008651** (.003704)
Relative Year [3]	.00804 (.005421)	.008275 (.006464)	.005047 (.005994)
Relative Year [ $\geq 4$ ]	.003512 (.004924)	.006228 (.004178)	.005626 (.004507)
Quarter-by-Census Division FE	Y	Y	Y
Focal Market Relative Year FE	Y	Y	Y
Four Lags of Own Log Price	Y	Y	Y
Neighbor-2 Relative Year FE	Y	Y	Y
Focal Market Fundamental Controls	Y	Y	Y
Log Average Price of Neighbor Groups	Y	Y	Y
Four Leads of Own Income	N	Y	Y
Four Leads of All Fundamental Controls	N	N	Y
N	4538	4142	4142

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic neighbor. Relative year [0] indicates the 12-month period preceding the boom of the neighboring MSA and is the omitted category. All specifications also include dummy variable(s) indicating whether the closest neighbor(s) are in the sample. Standard errors are clustered at the quarter by Census Division level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.7: The Impact of Nearest Neighbor Price Changes on Focal Market Price Changes, IV Results

<i>Dep. Var: Focal Market Price Changes</i>	
<i>Nearest Neighbor Price Changes</i>	
Relative Year [-2]	-0.045 (0.228)
Relative Year [-1]	0.041 (0.131)
Relative Year [1]	0.148*** (0.037)
Relative Year [2]	0.125*** (0.035)
Relative Year [3]	0.090** (0.037)
Relative Year [ $\geq 4$ ]	0.088*** (0.028)
Quarter-by-Census Division FE	Y
Focal Market Relative Year FE	Y
Four Lags of Focal Log Price	Y
Neighbor-2 Relative Year FE	Y
Instrumental Variable	Y
N	4131



Table 2.8: Summary Statistics on Lagged Number of Booms for the Hazard Estimation

	Lagged Number of New Booms				
	Mean	Sd	Min	Max	% Zero Boom
Neighbors [1-2]	0.0297	0.1777	0	2	97.20%
Neighbors [3-5]	0.0393	0.2058	0	3	96.20%
Neighbors [6-10]	0.0626	0.2692	0	3	94.30%
Neighbors [1-10]	0.1317	0.4246	0	5	89.00%
Neighbors [11-50]	0.3051	0.6079	0	7	76.20%
Neighbors [51+]	1.3055	1.6134	0	6	39.50%

Note: Neighbors are ranked with respect to their geographic distance from the focal MSA.

Table 2.9: Hazard Model Estimates of Neighbor Booms on the Probability of Focal Market Booming

	Unconditional Hazard	Baseline Results (Proportional Hazard)	Weibull Hazard
<i>Lagged Number of New Booms</i>	(1)	(2)	(3)
Neighbors[1-2]	3.49***	1.49	1.69*
Neighbors[3-5]	1.31	0.79	0.84
Neighbors[6-10]	1.41	1.26	1.25
Neighbors[11-50]	1.14	0.86	0.8
Neighbors[51+]	1.23***	1.02	1.05
Lagged % MSAs that Already Boomed	N	Y	Y
Focal Market Fundamental Controls	N	Y	Y
Four Lags of Focal Market Price Growth	N	Y	Y
Census Region FE	N	Y	Y
N	2114	2114	2114
Log-Likelihood	-29.93	29.43	34.14

Note: Implied Hazard Ratios are reported along with indicators of statistical significance of the underlying regression coefficients. Significance levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.10: Hazard Model Estimates of Neighbors on the Probability of Busting

	Unconditional Hazard	Proportional Hazard with Controls
<i>Lagged Number of New Busts</i>	(1)	(2)
Neighbors[1-2]	1.66*	0.96
Neighbors[3-5]	1.14	0.74
Neighbors[6-10]	1.55**	1.08
Neighbors[11-50]	1.12*	0.96
Neighbors[51+]	1.18***	1
Lagged % MSA that Already Busted	N	Y
Focal Market Fundamental Controls	N	Y
Four Lags of Focal Market Price Growth	N	Y
Census Region FE	N	Y
N	3,637	3,637
Log-Likelihood	2.181	100.1

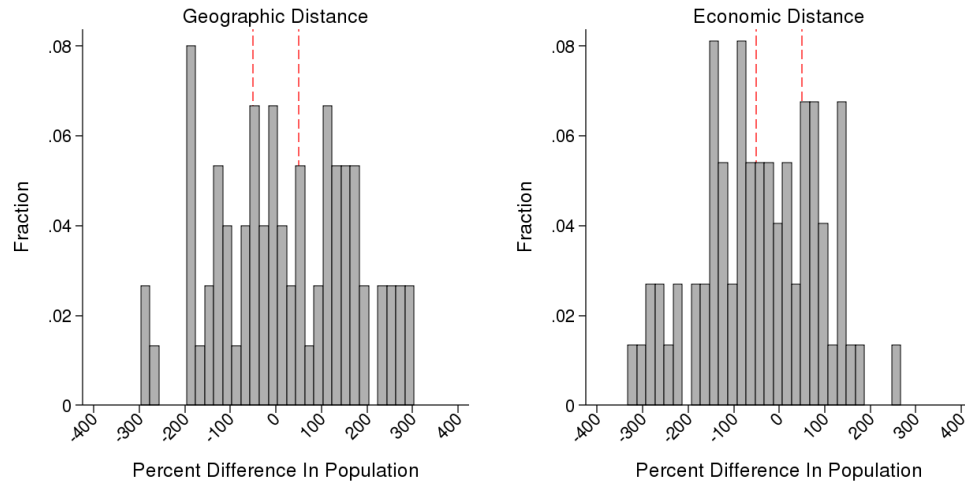
Note: We define the time of housing bust as the quarter when price peaks in our sample period. Implied hazard ratios are reported along with indicators of statistical significance of the underlying regression coefficients. Significance levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Table 2.11: The Impact of Nearest Neighbor Housing Bust on Focal Market Price

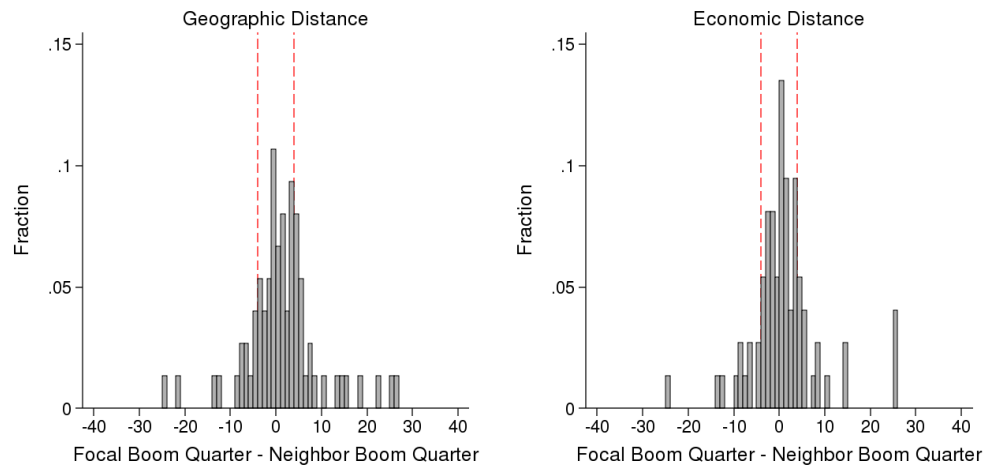
<i>Dep. Var: log(Focal Market Price)</i>	
<i>Nearest Neighbor Relative Years</i>	(1)
Relative Year [-2]	-.01169** (.004509)
Relative Year [-1]	-.003846 (.003101)
Relative Year [1]	-.006359 (.005119)
Relative Year [2]	-.01228** (.006151)
Relative Year [3]	-.03804*** (.007463)
Quarter-by-Census Division FE	Y
Focal Market Relative Year FE	Y
Four Lags of Focal Log Price	Y
Neighbor-2 Relative Year FE	Y
N	5178

Notes: Cells represent the coefficient on the dummy variable for indicated relative years of the closest geographic or economic neighbor. Relative year 0 indicates the 12 month period preceding the bust of the neighboring MSA and is the omitted category. All specifications also include dummy variable(s) indicating whether the closest neighbor(s) are in the sample. Standard errors are clustered at the MSA level and are reported in parentheses. Significance Levels 10%, 5%, and 1% are denoted by \*, \*\*, and \*\*\*, respectively.

Appendix Figure 2.A: Histogram of Percentage Difference in Population of Focal Markets and Nearest Neighbors



Appendix Figure 2.B: Histogram of Number of Quarters between Timing of the Booms of Focal Markets and Nearest Neighbors



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